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THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ADDRESS TO THE MATHEMATICAL AND PHYSICAL SECTION.*

The members of this Section will, I am sure, desire me to give expression to the gratification that we all feel in the realization of the scheme first proposed from this chair by Dr. Lodge, the scheme for the establishment of a national Physical Laboratory. It would be useless here to attempt to point out the importance of the step taken in the definite foundation of the Laboratory, for we all recognize that it was absolutely necessary for the due progress of physical research in this country. It is matter for congratulation that the initial guidance of the work of the Laboratory has been placed in such able hands.

While the investigation of nature is ever increasing our knowledge, and while each new discovery is a positive addition never again to be lost, the range of the investigation and the nature of the knowledge gained form the theme of endless discussion. And in this discussion, so different are the views of different schools of thought, that it might appear hopeless to look for general agreement, or to attempt to mark progress.

Nevertheless, I believe that in some directions there has been real progress,

*Given at the Dover meeting on September 14, 1899, by the President of the Section.

and that physicists, at least, are tending towards a general agreement as to the nature of the laws in which they embody their discoveries, of the explanations which they seek to give, and of the hypotheses they make in their search for explanations.

I propose to ask you to consider the terms of this agreement, and the form in which, as it appears to me, they should be drawn up.

The range of the physicist's study consists in the visible motions and other sensible changes of matter. The experiences with which he deals are the impressions on his senses, and his aim is to describe in the shortest possible way how his various senses have been, will be, or would be affected.

His method consists in finding out all likenesses, in classing together all similar events, and so giving an account as concise as possible of the motions and changes observed. His success in the search for likenesses and his striving after conciseness of description lead him to imagine such a constitution of things that likenesses exist even where they elude his observation, and he is thus enabled to simplify his classification on the assumption that the constitution thus imagined is a reality. He is enabled to predict on the assumption that the likenesses of the future will be the likenesses of the past.

His account of Nature, then, is, as it is often termed, a descriptive account.

Were there no similarities in events, our account of them could not rise above a mere directory, with each individual event entered up separately with its address. But the similarities observed enable us to class large numbers of events together, to give general descriptions, and indeed to make, instead of a directory, a readable book of science, with laws as the headings of the chapters.

These laws are, I believe, in all cases brief descriptions of observed similarities. By way of illustration let us take two or three examples.

The law of gravitation states that to each portion of matter we can assign a constant —its mass—such that there is an acceleration towards it of other matter proportional to that mass divided by the square of its distance away. Or all bodies resemble each other in having this acceleration towards each other.

Hooke's law for the case of a stretched wire states that each successive equal small load produces an equal stretch, or states that the behavior of the wire is similar for all equal small pulls.

Joule's law for the heat appearing when a current flows in a wire states that the rate of heat development is proportional to the square of the current multiplied by the resistance, or states that all the different cases resemble each other in having $H \div C^2Rt$ constant.

And, generally, when a law is expressed by an equation, that equation is a statement that two different sets of measurements are made, represented by the terms on the two sides of the equation, and that all the different cases resemble each other in that the two sets have the constant relation expressed by the equation. Accurate prediction is based on the assumption that when we have made the measurements on the one side of the equation we can tell the result of the measurements implied on the other side.

If this is a true account of the nature of physical laws, they have, we must confess, greatly fallen off in dignity. No long time ago they were quite commonly described as the Fixed Laws of Nature, and were supposed sufficient in themselves to govern the universe. Now we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed.

The old conception of laws as self-sufficing governors of Nature was, no doubt, a

survival of a much older conception of the scope of physical science, a mode of regarding physical phenomena which has itself passed away.

I imagine that originally man looked on himself and the result of his action in the motions and changes which he produced in matter, as the one type in terms of which he should seek to describe all motions and changes. Knowing that his purpose and will were followed by motions and changes in the matter about him, he thought of similar purpose and will behind all the motions and changes which he observed, however they occurred ; and he believed, too, that it was necessary to think thus in giving any consistent account of his observations. Taking this anthropomorphic—or, shall we say, psychical—view, the laws he formulated were not merely descriptions of similarities of behavior, but they were also expressions of fixed purpose and the resulting constancy of action. They were commands given to matter which it must obey.

The psychical method, the introduction of purpose and will, is still appropriate when we are concerned with living beings. Indeed, it is the only method which we attempt to follow when we are describing the motions of our fellow-creatures. No one seeks to describe the motions and actions of himself and of his fellow-men, and to classify them without any reference to the similarity of purpose when the actions are similar. But as the study of Nature progressed, it was found to be quite futile to bring in the ideas of purpose and will when merely describing and classifying the motions and changes of non-living matter. Purpose and will could be entirely left out of sight, and yet the observed motions and changes could be described, and predictions could be made as to future motions and changes. Limiting the aim of physical science to such description and prediction, it gradually became clear that the method was adequate for the purpose, and over the range of non-living matter, at least, the psychical yielded to the physical. Laws ceased to be commands analogous to legal enactments, and became mere descriptions. But during the passage from one position to the other, by a confusion of thought which may appear strange to us now that we have finished the journey, though no doubt it was inevitable, the purpose and will of which the laws had been the expression were put into the laws themselves; they were personified and made to will and act.

Even now these early stages in the history of thought can be traced by survivals in our language, survivals due to the ascription of moral qualities to matter. Thus gases are still sometimes said to obey or to disobey Boyle's law as if it were an enactment for their guidance, and as if it set forth an ideal, the perfect gas, for their imitation. We still hear language which seems to imply that real gases are wanting in perfection, in that they fail to observe the exact letter of the law. I suppose on this view we should have to say that hydrogen is nearest to perfection ; but then we should have to regard it as righteous overmuch, a sort of Pharisee among gases which overshoots the mark in its endeavor to obey the law. Oxygen and nitrogen we may regard as good enough in the affairs of everyday life. But carbon dioxide and chlorine and the like are poor sinners which yield to temptation and liquefy whenever circumstances press at all hardly on them.

There is a similar ascription of moral qualities when we judge bodies according to their fulfillment of the purpose for which we use them, when we describe them as good or bad radiators, good or bad insulators, as if it were a duty on their part to radiate well, or insulate well, and as if there were failures on the part of Nature to come up to the proper standard.

These are of course mere trivialities, but

the reaction of language on thought is so subtle and far-reaching that, risking the accusation of pedantry, I would urge the abolition of all such picturesque terms. In our quantitative estimates let us be content with 'high' or 'low,' 'great' or 'small,' and let us remember that there is no such thing as a failure to obey a physical law. A broken law is merely a false description.

Concurrently with the change in our conception of physical law has come a change in our conception of physical explanation. We have not to go very far back to find such a statement as this—that we have explained anything when we know the cause of it, or when we have found out the reason why—a statement which is only appropriate on the psychical view. Without entering into any discussion of the meaning of cause, we can at least assert that that meaning will only have true content when it is concerned with purpose and will. On the purely physical or descriptive view, the idea of cause is quite out of place. In description we are solely concerned with the 'how' of things, and their 'why' we purposely leave out of account. We explain an event not when we know 'why' it happened, but when we show 'how' it is like something else happening elsewhere or otherwhen—when, in fact, we can include it as a case described by some law already set forth. In explanation, we do not account *for* the the event, but we improve our account *of* it by likening it to what we already knew.

For instance, Newton explained the falling of a stone when he showed that its acceleration towards the earth was similar to and could be expressed by the same law as the acceleration of the moon towards the earth.

He explained the air disturbance we call 'sound' when he showed that the motions and forces in the pressure waves were like motions and forces already studied.

Franklin explained lightning when and so far as he showed that it was similar in its behavior to other electric discharges.

Here I do not fear any accusation of pedantry in joining those who urge that we should adapt our language to the modern view. It would be a very real gain, a great assistance to clear thinking, if we could entirely abolish the word 'cause' in physical description, cease to say 'why' things happen unless we wish to signify an antecedent purpose, and be content to own that our laws are but expressions of 'how' they occur.

The aim of explanation, then, is to reduce the number of laws as far as possible, by showing that laws, at first separated, may be merged in one; to reduce the number of chapters in the book of science by showing that some are truly mere sub-sections of chapters already written.

To take an old but never-worn-out metaphor, the physicist is examining the garment of Nature, learning of how many, or rather of how few different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come.

How many different kinds of thread does Nature use?

So far, we have recognized some eight or nine, the number of different forms of energy which we are still obliged to count as distinct. But this distinction we cannot believe to be real. The relations between the different forms of energy, and the fixed rate of exchange when one form gives place to another, encourage us to suppose that if we could only sharpen our senses, or change our point of view, we could effect a still further reduction. We stand in front of Nature's loom as we we watch the weaving of the garment; while we follow a particular thread in the pattern it suddenly disappears, and a thread of another color takes its place. Is this a new thread, or is

it merely the old thread turned round and presenting a new face to us? We can do little more than guess. We cannot get to the other side of the pattern, and our minutest watching will not tell us all the working of the loom.

Leaving the metaphor, were we true physicists, and physicists alone, we should, I suppose, be content to describe merely what we observe in the changes of energy. We should say, for instance, that so much kinetic energy ceases, and that so much heat appears, or that so much light comes to a surface, and that so much chemical energy takes its place. But we have to take ourselves as we are, and reckon with the fact that though our material is physical, we ourselves are psychical. And, as a mere matter of fact, we are not content with such discontinuous descriptions. We dislike the discontinuity and we think of an underlying identity. We think of the heat as being that which a moment before was energy of a visible motion, we think of the light as changing its form alone and becoming itself the chemical energy. Then to our passive dislike to discontinuity we join our active desire to form a mental picture of what may be going on, a picture like something which we already know. Coming on these discontinuities our ordinary method of explanation fails, for they are not obviously like those series of events in which we can trace every step. We then imagine a constitution of matter and modifications of it corresponding to the different kinds of energy, such that the discontinuities vanish, and such that we can picture one form of energy passing into another and yet keeping the same in kind throughout. We are no longer content to describe what we actually see or feel, but we describe what we imagine we should see or feel if our senses were on quite another scale of magnitude and sensibility. We cease to be physicists of the real and become physicists of the ideal.

To form such mental pictures we naturally choose the sense which makes such pictures most definite, the sense of sight, and think of a constitution of matter which shall enable us to explain all the various changes in terms of visible motions and accelerations. We imagine a mechanical constitution of the universe.

We are encouraged in this attempt by the fact that the relations in this mechanical conception can be so exactly stated, that the equations of motion are so very definite. We have, too, examples of mechanical systems, of which we can give accounts far exceeding in accuracy the accounts of other physical systems. Compare, for instance, the accuracy with which we can describe and foretell the path of a planet with our ignorance of the movements of the atmosphere as dependent on the heat of the sun. The planet keeps to the astronomer's time table, but the wind still bloweth almost where it listeth.

The only foundation which has yet been imagined for this mechanical explanation—if we may use 'explanation' to denote the likening of our imaginings to that which we actually observe—is the atomic and molecular hypothesis of matter. This hypothesis arose so early in the history of science that we are almost tempted to suppose that it is a necessity of thought, and that it has a warrant of some higher order than any other hypothesis which could be imagined. But I suspect that if we could trace its early development we should find that it arose in an attempt to explain the phenomena of expansion and contraction, evaporation and solution. Were matter a continuum we should have to admit all these as simple facts, inexplicable in that they are like nothing else. But imagine matter to consist of a crowd of separate particles with interspaces. Contraction and expansion are then merely a drawing in and a widening out of the crowd. Solu-

tion is merely the mingling of two crowds, and evaporation merely a dispersal from the outskirts. The most evident properties of matter are then similar to what may be observed in any public meeting.

For ages the molecular hypothesis hardly went further than this. The first step onward was the ascription of vibratory motion to the atoms to explain heat. Then definite qualities were ascribed, definite mutual forces were called into play to explain elasticity and other properties or qualities of matter. But I imagine its first really great achievement was its success in explaining the law of combining proportions, and next to that we should put its success in explaining many of the properties of gases.

While light was regarded as corpuscular —in fact molecular, and while direct action at a distance presented no difficulty, the molecular hypothesis served as the one foundation for the mechanical representation of phenomena. But when it was shown that infinitely the best account of the phenomena of light could be given on the supposition that it consisted of waves, something was needed, as Lord Salisbury has said, to wave, both in the interstellar and in the intermolecular spaces. So the hypothesis of an ether was developed, a necessary complement of that form of the molecular hypothesis in which matter consists of discrete particles with matter-free intervening spaces.

Then Faraday's discovery of the influence of the dielectric medium in electric actions led to the general abandonment of the idea of action at a distance, and the ether was called in to aid matter in the explanation of electric and magnetic phenomena. The discovery that the velocity of electro-magnetic waves is the same as that of light waves is at least circumstantial evidence that the same medium transmits both.

I suppose we all hope that some time we shall succeed in attributing to this medium such further qualities that it will be able to enlarge its scope and take in the work of gravitation.

The mechanical hypothesis has not always taken this dualistic form of material atoms and molecules, floating in a quite distinct ether. I think we may regard Boscovich's theory of point-centers surrounded by infinitely extending atmospheres of force as really an attempt to get rid of the dualism, and Faraday's theory of point-centers with radiating lines of force is only Boscovich's theory in another form. But Lord Kelvin's vortex-atom theory gives us a simplification more easily thought of. Here all space is filled with continuous fluid —shall we say a fluid ether—and the atoms are mere loci of a particular type of motion of this frictionless fluid. The sole differences in the atoms are differences of position and motion. Where there are whirls, we call the fluid matter; where there are no whirls we call it ether. All energy is energy of motion. Our visible kinetic energy, $MV^2/2$, is energy in and around the central whirls; our visible energy of position, our potential energy, is energy of motion in the outlying regions.

A similar simplification is given by Dr. Larmor's hypothesis, in which, again, all space is filled with continuous substance all of one kind, but this time solid rather than fluid. The atoms are loci of strain instead of whirls, and the ether is that which is strained.

So, as we watch the weaving of the garment of Nature, we resolve it in imagination into threads of ether spangled over with beads of matter. We look still closer, and the beads of matter vanish; they are mere knots and loops in the threads of ether.

The question now faces us—How are we to regard these hypotheses as to the consti-

tution of matter and the connecting ether? How are we to look upon the explanations they afford? Are we to put atoms and ether on an equal footing with the phenomena observed by our senses, as truths to be investigated for their own sake? Or are they mere tools in the search for truth, liable to be worn out or superseded?

That matter is grained in structure is hardly more than the expression of the fact that in very thin layers it ceases to behave as in thicker layers. But when we pass on from this general statement and give definite form to the granules or assume definite qualities to the intergranular cement, we are dealing with pure hypotheses.

It is hardly possible to think that we shall ever see an atom or handle the ether. We make no attempt whatever to render them evident to the senses. We connect observed conditions and changes in gross visible matter by invisible molecular and ethereal machinery. The changes at each end of the machinery of which we seek to give an account are in gross matter, and this gross matter is our only instrument of detection, and we never receive direct sense impressions of the imagined atoms or the intervening ether. To a strictly descriptive physicist their only use and interest would lie in their service in prediction of the changes which are to take place in gross matter.

It appears quite possible that various types of machinery might be devised to produce the known effects. The type we have adopted is undergoing constant minor changes, as new discoveries suggest new arrangements of the parts. Is it utterly beyond possibility that the type itself should change?

The special molecular and ethereal machinery which we have designed, and which we now generally use, has been designed because our most highly developed sense is our sense of sight. Were we otherwise, had we a sense more delicate than sight, one affording us material for more definite mental presentation, we might quite possibly have constructed very different hypotheses. Though, as we are, we cannot conceive any higher type than that founded on the sense of sight, we can imagine a lower type, and by way of illustration of the point let us take the sense of which my predecessor spoke last year—the sense of smell. In us it is very undeveloped. But let us image a being in whom it is highly cultivated, say, a very intellectual and very hypothetical dog. Let us suppose that he tries to frame an hypothesis as to light. Having found that his sense of smell is excited by surface exhalations, will he not naturally make and be content with a corpuscular theory of light? When he has discovered the facts of dispersion, will he not think of the different colors as different kinds of smell—insensible, perhaps, to him, but sensible to a still more highly gifted, still more hypothetical dog?

Of course, with our superior intellect and sensibility, we can see where his hypothesis would break down; but unless we are to assume that we have reached finality in sense development, the illustration, grotesque as it may be, will serve to show that our hypotheses are in terms of ourselves rather than in terms of Nature itself, they are ejective rather than objective, and so they are to be regarded as instruments, tools, apparatus only to aid us in the search for truth.

To use an old analogy—and here we can hardly go except upon analogy—while the building of Nature is growing spontaneously from within, the model of it, which we seek to construct in our descriptive science, can only be constructed by means of scaffolding from without, a scaffolding of hypotheses. While in the real building all is continuous, in our model there are detached parts which must be connected with the rest by tem-

porary ladders and passages, or which must be supported till we can see how to fill in the understructure. To give the hypotheses equal validity with facts is to confuse the temporary scaffolding with the building itself.

But even if we take this view of the temporary nature of our molecular and ethereal imaginings, it does not lessen their value, their necessity to us.

It is merely a true description of ourselves to say that we must believe in the continuity of physical processes, and that we must attempt to form mental pictures of those processes, the details of which elude our observation. For such pictures we must frame hypotheses, and we have to use the best material at command in framing them. At present there is only one fundamental hypothesis—the molecular and ethereal hypothesis—in some such form as is generally accepted.

Even if we take the position that the form of the hypothesis may change as our knowledge extends, that we may be able to devise new machinery—nay, even that we may be able to design some quite new type to bring about the same ends—that does not appear to me to lessen the present value of the hypothesis. We can recognize to the full how well it enables us to group together large masses of facts which, without it, would be scattered apart, how it serves to give working explanations, and continually enables investigators to think out new questions for research. We can recognize that it is the symbolical form in which much actual knowledge is cast. We might almost as well quarrel with the use of the letters of the alphabet, inasmuch as they are not the sounds themselves, but mere arbitrary symbols of the sounds.

In this country there is no need for any defence of the use of the molecular hypothesis. But abroad the movement from the position in which hypothesis is confounded with observed truth has carried many through the position of equilibrium equally far on the other side, and a party has been formed which totally abstains from molecules as a protest against immoderate indulgence in their use. Time will show whether these protesters can do without any hypothesis, whether they can build without scaffolding or ladders. I fear that it is only an attempt to build from balloons.

But the protest will have value if it will put us on our guard against using molecules and the ether everywhere and everywhen. There is, I think, some danger that we may get so accustomed to picturing everything in terms of these hypotheses that we may come to suppose that we may have no firm basis for the facts of observation until we have given a molecular account of them, that a molecular basis is a firmer foundation than direct experience.

Let me illustrate this kind of danger. The phenomena of capillarity can, for the most part, be explained on the assumption of a liquid surface tension. But if the subject is treated merely from this point of view it stands alone—it is a portion of the building of science hanging in the air. The molecular hypothesis then comes in to give some explanation of the surface tension, gives, as it were, a supporting understructure connecting capillarity with other classes of phenomena. But here, I think, the hypothesis should stop, and such phenomena as can be explained by the surface tension should be so explained without reference to molecules. They should not be brought in again till the surface-tension explanation fails. It is necessary to bear in mind what part is scaffolding, and what is the building itself, already firm and complete.

Or, as another illustration, take the Second Law of Thermodynamics. I suspect that it is sometimes supposed that a molecular theory from which the Second Law

could be deduced would be a better basis for it than the direct experience on which it was founded by Clausius and Kelvin or, that the mere imagining of a Maxwell's sorting demon has already disproved the universality of the law; whereas he is a mere hypothesis grafted on a hypothesis, and nothing corresponding to his action has yet been found.

There is more serious danger of confusion of hypothesis with fact in the use of the ether; more risk of failure to see what is accomplished by its aid. In giving an account of light, for instance, the right course, it appears to me, is to describe the phenomena and lay down the laws under which they are grouped, leaving it an open question what it is that waves, until the phenomena oblige us to introduce something more than matter, until we see what properties we must assign to the ether, properties not possessed by matter, in order that it may be competent to afford the explanations we seek. We should then realize more clearly that it is the constitution of matter which we have imagined, the hypothesis of discrete particles, which obliges us to assume an intervening medium to carry on the disturbance from particle to particle. But the vortex-atom hypothesis and Dr. Larmor's strain-atom hypothesis both seem to indicate that we are moving in the direction of the abolition of the distinction between matter and ether, that we shall come to regard the luminiferous medium, not as an attenuated substance here and there encumbered with detached blocks—the molecules of matter—but as something which in certain places exhibits modifications which we term matter. Or starting rather from matter, we may come to think of matter as no longer consisting of separated granules, but as a continuum with properties grouped round the centers, which we regard as atoms or molecules.

Perhaps I may illustrate the danger in the use of the conception of the ether by considering the common way of describing the electro-magnetic waves, which are all about us here, as ether waves. Now in all cases with which we are acquainted, these waves start from matter; their energy before starting was, as far as we can guess, energy of the matter between the different parts of the source, and they manifest themselves in the receiver as energy of matter. As they travel through the air, I believe that it is quite possible that the electric energy can be expressed in terms of the molecules of air in their path, that they are effecting atomic separations as they go. If so, then the air is quite as much concerned in their propagation as the ether between its molecules. In any case, to term them ether waves is to prejudge the question before we have sufficient evidence.

Unless we bear in mind the hypothetical character of our mechanical conception of things, we may run some risk of another danger—the danger of supposing that we have something more real in mechanical than in other measurements. For instance, there is some risk that the work measure of specific heat should be regarded as more fundamental than the heat measure, in that heat is truly a 'mode of motion.' On the molecular hypothesis, heat is no doubt a mixture of kinetic energy and potential energy of the molecules and their constituents, and may even be entirely kinetic energy; and we may conceivably in the future make the hypothesis so definite that, when we heat a gramme of water 1°, we can assign such fraction of an erg to each atom. But look how much pure hypothesis is here. The real superiority of the work measure of specific heat lies in the fact that it is independent of any particular substance, and there is nothing whatever hypothetical about it.*

* This risk of imagining one particular kind of

Another illustration of the illegitimate use of our hypothesis, as it appears to me, is in the attempt to find in the ether a fixed datum for the measurement of material velocities and accelerations, a something in which we can draw our coördinate axes so that they will never turn or bend. But this is as if, discontented with the movement of the earth's pole, we should seek to find our zero lines of latitude and longitude in the Atlantic Ocean. Leaving out of sight the possibility of ethereal currents which we cannot detect, and the motions due to every ray of light which traverses space, we could only fix positions and directions in the ether by buoying them with matter. We know nothing of the ether, except by its effects on matter, and, after all, it would be the material buoys which would fix the positions and not the ether in which they float.

The discussion of the physical method, with its descriptive laws and explanations, and its hypothetical extension of description, leads us on to the consideration of the limitation of its range. The method was developed in the study of matter which we describe as non-living, and with non-living matter the method has sufficed for the particular purposes of the physicist. Of course only a little corner of the universe has been explored, but in the study of non-living matter we have come to no impassable gulfs, no chasms across which we cannot throw bridges of hypothesis. Does the method equally suffice when it is applied to living matter? Can we give a purely physical account of such matter, likening its motions and changes to other motions and changes already observed, and so explaining them? Can we group them in laws which will enable us to predict future conditions and positions?—The ancient question never answered, but never ceasing to press for an answer.

Having faith in our descriptive method, let us use it to describe our real attitude on the question. Do we, or do we not, as a matter of fact, make any attempt to apply the physical method to describe and explain those motions of matter which on the psychical view we term voluntary?

Any commonplace example, and the more commonplace the more it is to the point, will at once tell us our practice, whatever may be our theory. For instance, a steamer is going across the Channel. We can give a fairly good physical account of the motion of the steamer. We can describe how the energy stored in the coal passes out through the boiler into the machinery, and how it is ultimately absorbed by the sea. And the machinery once started, we can give an account of the actions and reactions between its various parts and the water, and if only the crew will not interfere, we can predict with some approach to correctness how the vessel will run. All these processes can be likened to processes already studied—perhaps on another scale—in our laboratories, and from the similarities prediction is possible. But now think of a passenger on board who has received an invitation to take the journey. It is simply a matter of

measure more real than another, more in accordance with the truth of things, may be further illustrated by the common idea that mass-acceleration is the only way to measure a force. We stand apart from our mechanical system and watch the motions and the accelerations of the various parts, and we find that mass-accelerations have a certain significance in our system. If we keep ourselves outside the system and only use our sense of sight, then mass-acceleration is the only way of describing that behavior of one body in the presence of others which we term force on it. But if we go about in the system and pull and push bodies, we find that there is another conception of force, in which another sense than sight is concerned —another mode of measurement much more ancient and still far more extensively used—the measurement by weight supported. Each method has its own range; each is fundamental in that range. It is one of the great practical problems in physics to make the pendulum give us the exact ratio of the units in the two systems.

fact that we make no attempt at a complete physical account and explanation of those actions which he takes to accomplish his purpose. We trace no lines of induction in the ether connecting him with his friends across the Channel, we seek no law of force under which he moves. In practice the strictest physicist abandons the physical view, and replaces it by the psychical. He admits the study of purpose as well as the study of motion.

He has to admit that here his physical method of prediction fails. In physical observations one set of measurements may lead to the prediction of the results of another set of measurements. The equations expressing the laws imply different observations with some definite relation between their results, and if we know one set of observations and that definite relation we can predict the result of the other set. But if we take the psychical view of actions, we can only measure the actions. We have no independent means of studying and measuring the motions which preceded the actions, we can only estimate their value by the consequent actions. If we formed equations they would be mere identities with the same terms on either side.

The consistent and persistent physicist, finding the door closed against him, finding that he has hardly a sphere of influence left to him in the psychical region, seeks to apply his methods in another way, by assuming that if he knew all about the molecular positions and motions in the living matter, then the ordinary physical laws could be applied, and the physical conditions at any future time could be predicted. He would say, I suppose, with regard to the Channel passenger, that it is absurd to begin with the most complicated mechanism, and seek to give a physical account of that. He would urge that we should take some lower form of life where the structure and motions are simpler, and apply the physical methods to that.

Well, then, let us look for the physical explanation of any motion which we are entitled from its likeness to our own action to call a voluntary motion. Must we not own that even the very beginning of such explanation is as yet non-existent? It appears to me that the assumption that our methods do apply, and that purely physical explanation will suffice to predict all motions and changes, voluntary and involuntary, is at present simply a gigantic extrapolation, which we should unhesitatingly reject if it were merely a case of ordinary physical investigation. The physicist when thus extending his range is ceasing to be a physicist, ceasing to be content with his descriptive methods in his intense desire to show that he is a physicist throughout.

Of course we may describe the motions and changes of any type of matter after the event, and in a purely physical manner. And as Professor Ward has suggested, in a most important contribution to this subject which he has made in his recently published Gifford Lectures,* where ordinary physical explanations fail to give an account of the motions, we might imagine some structure in the ether, and such stresses between the ether and matter that our physical explanations should still hold. But, as Professor Ward says, such ethereal constructions would present no warrant for their reality or consistency. Indeed they would be mere images in the surface of things to account for what goes on in front of the surface, and would have no more reality than the images of objects in a glass.

If we have full confidence in the descriptive method, as applied to living and non-living matter, it appears to me that up to the present it teaches us that while in non-living matter we can always find similarities, that, while each event is like other events, actual or imagined, in a living being there

* 'Naturalism and Agnosticism,' *The Gifford Lectures*, 1896–98, Vol II., p. 71.

are always dissimilarities. Taking the psychical view—the only view which we really do at present take—in the living being there is always some individuality, something different from any other living being, and full prediction in the physical sense, and by physical methods is impossible. If this be true, the loom of Nature is weaving a pattern with no mere geometrical design. The threads of life, coming in we know not where, now twining together, now dividing, are weaving patterns of their own, ever increasing in intricacy, ever gaining in beauty.

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*THE WORK OF THE INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.**

PRIOR to the year 1800 little was known of the properties of the materials of construction. Gallileo had shown in 1638 that the strength of a rectangular beam varied with the square of its depth; Hooke in 1678 had announced the law that the stretch of a spring was proportional to the stress upon it; various authors had discussed the forms of beams of uniform strength, and Euler, in 1744, had enunciated his formula for the resistance of columns under compression. Theory was far in advance of practice, for experiments had been so few and so imperfect that the elastic limit was scarcely recognized.

During the years from 1800 to 1850 great progress was made in the theory of elasticity, and a slow growth took place in knowledge of the properties of materials under stress. The introduction of railways and the consequent necessity of providing a firm roadbed and safe bridge structures gave a powerful stimulus to the investigation of metals, in order that ample security might be afforded with the greatest degree of economy. The methods of testing were, however, so imperfect that progress was slow, and, with the exception of the classic researches of Hodgskinson, the work of this period was mostly of value as a preparation for that of the future.

After 1850 large testing machines for special purposes began to be built, elongation and ductility began to be carefully studied, and soon after 1870, it was recognized by many manufacturers that physical tests of metals were imperatively necessary in order to secure uniformity of product. As these tests were multiplied and the records subjected to investigation, the knowledge was gained that the strength of a specimen depended upon its size and proportions and also upon the manner in which the load was applied. The term elastic limit assumed a new significance when it became recognized that it could be defined and measured in different ways. In short, it was found that tests of materials must be made in a similar manner in order to render the results comparable. This idea, although long recognized, has proved a difficult one to realize. It has been discussed by many engineering societies, some of which have attempted to formulate standard methods. Finally the International Association for Testing Materials was formed in order to study the whole subject and endeavor to arrive at conclusions that should be authoritative.

In 1882, through the influence of John Bauschinger, a number of German experimenters met at Munich and discussed the question as to how uniformity in the methods of testing materials could be promoted. As a result of this meeting, formal conferences were held at Dresden in 1884, at Berlin in 1886, at Munich in 1888, and at Vienna in 1893, delegates from other European countries being often present. The reports of the proceedings of these conferences, published in Bauschinger's *Mittheilungen*, attracted wide attention, and the

* An address by the Chairman of the American Section of the Association, at the second annual meeting held in Pittsburg, Pa., August 15-16, 1899.

great value and importance of the discussions became universally recognized in engineering circles. In short, the movement assumed an international character.

In 1890, as a result of the International Congresses of Engineering, held at Paris, in the preceding year, the French government appointed a commission to formulate standard methods for testing the materials of construction. Its report, published in 1894, in four large volumes, is one of the most valuable contributions to the subject, but from the first it was recognized that ultimate conclusions could not be determined by a commission of one nationality, and accordingly, since 1895, the French government has given hearty support to the work of the International Association.

In 1895, as a result of the four preceding conferences, the fifth conference met at Zurich, all European countries, except Turkey, being represented. The United States Government was represented by an army officer, and the American Society of Mechanical Engineers by a delegate. At this Congress the International Association for Testing Materials was formally organized, its object being, as stated in its statutes, "the development and unification of standard methods of testing for the determination of the properties of the materials of construction, and of other materials, and also the perfection of apparatus for that purpose." This meeting at Zurich hence assumed an importance far greater than any preceding conference, and it may be called the first Congress of the International Association.

At the Vienna convention of 1893 there had been appointed twenty committees on technical subjects, and reports from many of these were presented at the Zurich Congress of 1895. These reports were published in the French and German languages in the official organ of the Association called *Baumaterialienkunde*, the first number of which appeared in July, 1896. The work of some of these committees was continued, other subjects were proposed for future consideration, and a council was organized to transact the business of the International Association in the intervals between the Congresses.

In 1897 the second Congress of the International Association was held at Stockholm, there being present 361 members representing 18 countries. The United States Government was represented by an army officer and a navy officer, and the American Society of Mechanical Engineers by a delegate. The Congress continued in session for three days, reports of committees were presented, papers read and discussed, and plans outlined for future work. It was resolved that the next Congress should be held in Paris in the summer of 1900, and the Council was authorized to appoint technical committees to make reports at that time on special problems relating to the objects of the Association.

At a meeting of the International Council held early in 1898, appointments were made of chairmen of 21 committees on technical problems, and the number of members on each committee from each country was assigned. It was also recommended, in order to expedite the appointment and work of these committees, that the members in each country should meet and form a national section of the International Association. In compliance with this recommendation a number of the American members met on June 15, 1898, and organized an American Section, whose first annual meeting was held at Philadelphia on August 27, 1898, and whose second annual meeting I now have the honor to address.

The membership of the International Association numbered 493 in 1895 ; 953 in 1896; 1,169 in 1897; 1,488 in 1898, and is now probably about 2,000. Germany takes the lead in regard to number of members, it

having 387 in 1898, while Russia had 315, Austria 158, England 83, Switzerland 83, United States 68, Sweden 68, France 66, Holland 48, Norway 42, Denmark 39, Spain 36, Italy 35, and 60 from nine other countries. With regard to the American membership, it may be noted that it numbered 6 in 1895; 25 in 1896; 60 in 1897; 68 in 1898, prior to the organization of the American Section; 106 in February, 1899, and that it is now nearly or quite 125.

There are two peculiarities regarding membership in this Association that deserve notice. First, there is no nomination or election of members, but any person desiring to be a member may do so on signing a statement that he assumes membership and will be governed by the laws of the Association; in so doing he further assumes the obligation, stated in Art. 5, of the statutes, that he will advance its interests to the best of his ability. Membership is hence a voluntary act assumed by an individual in order to promote the knowledge of the properties of materials and to endeavor to secure uniformity in methods of testing them. Withdrawal from membership may be made at any time by mere announcement to the proper officer of the Association.

The second noteworthy feature regarding membership is that it may be assumed by a corporation or society as well as by a person. For example, in the list of members of the American Section, published in February last, will be found the Franklin Institute, the American Society of Mechanical Engineers, the American Foundrymen's Association, and five local engineering clubs, as also several steel companies, engineering journals, and firms engaged in inspecting and testing. In Europe this feature is carried much further, the membership of the German Section including the bureau of public works of several cities, provinces and states, the police bureau of Berlin, the Prussian war department and the boards of direction of numerous railways, as also a large number of manufacturing corporations and engineering societies. Under this arrangement it is possible for a corporation to exert a greater influence than through the indirect individual membership of its president or superintendent, both manufacturers and consumers can make their wishes more directly known, and thus differences in regard to methods of inspection and testing can be more quickly harmonized than under the usual plan of strict individual membership. However, fully three-fourths of the total members are individuals, and these include engineers in all branches, architects, chemists, professors of mechanics and engineering, and superintendents and foremen of works.

At the Zurich Congress the dues of members were fixed at $1.00 per year, and while no change was made at the Stockholm Congress, the Council recommended early in 1898, in view of the heavy expenses, that each member should pay $1.50 per year. Accordingly, at the first annual meeting of this Section, when our present by-laws were adopted, the provision was inserted that each member should pay $2.50 per year, of which $1.50 should be transmitted to the International Association and the remainder be used to defray the expenses of the American Section. This by-law went into effect on January 1, 1899, and accordingly no dues were collected by this Section for the year 1898, the $1.50 payable for that year being forwarded to the International Council directly by each member or through the American member of that Council. During the present year dues have been paid directly to our secretary, and his report, together with that of our treasurer, will be laid before this meeting.

The dues of $1.50 per year per member, transmitted to the International Council,

are used by it in issuing its publications and in assisting its committees in defraying a part of the expenses of their special investigations. In addition to this income a number of societies and bureaus have agreed to make extra annual contributions, the Prussian War Department heading the list with $125, and 21 others giving smaller sums, so that for the year 1898 the amount derived from these sources was about $400. Although official information is not at hand, it is safe to say that the total income of the International Association for the year 1898 did not exceed $2,000, which is certainly a small sum with which to issue its publications and carry on the work of 21 committees.

The International Association has issued yearly, since 1895, a list of members, and also abstracts of the proceedings of the Congresses of 1895 and 1897. These, together with a few circulars of information, constitute all the publications that it has been able to furnish free to its members. The detailed proceedings of the Congresses have been printed in the journal *Baumaterialienkunde* published in the French and German languages, at Stuttgart, which has been furnished to members at $2.50 per year, the regular subscription price being $3.50. It will be seen, therefore, that an American member who desires to be fully informed regarding the work of the Association must necessarily subscribe to this Journal, and by so doing his dues become really $5 per year. It should further be stated that arrangements will probably be made so that the official announcements of the International Council and the proceedings of future Congresses will be printed in this Journal in the English language, as well as in German and French.

The American Section, as already stated, had no income during 1898, and the report of our Treasurer shows that during the present year the amount available for expenses has been about $120. On February 18, a pamphlet of twenty-six pages was issued containing a list of officers of the International Association and its committees and a list of the American members, together with the statutes, by-laws and some historical information. In April a bulletin was issued giving abstracts of the proceedings of the first annual meeting and of the meetings of the executive committee, and in July a second bulletin was issued containing the preliminary programme for this meeting. It is hoped that the condition of our treasury may permit these bulletins to be continued, and that one may be issued containing the proceedings of this meeting.

The technical questions proposed for discussion at the Paris Congress of 1900 are nineteen in number. The organization of the international committees which are to consider these topics is now complete, and preliminary reports from the American members of several of them are to be presented and discussed at this meeting. Probably the most important of these subjects is that of standard international specifications for testing and inspecting iron and steel; this committee originally consisted of about forty members, of which five were assigned to this country, but under authority to increase its numbers the American sub-committee has been increased to twenty-one, has held several meetings, collected specifications and will present a preliminary report of much interest. It is also expected that the American members of five other international committees on iron and steel will report progress in their organization and work. As the national sub-committees are now in full correspondence with the international chairmen, it is expected that the final reports which are to be presented for discussion at the Paris Congress will prove of great interest and value.

Of the nineteen problems to be considered by the nineteen international committees,

six are on iron and steel, one on stone and slate, eight on cements and mortars, one on tile pipes, one on paints, one on lubricants, and one on the dry rot of wood. The fact that there are eight committees on cements and mortars and only six on iron and steel may seem abnormal, but it should be remembered that in the testing of hydraulic cement the personal equation of the observer enters to a far greater degree than in the case of metals, and that its rapidly increasing use demands the immediate perfection of methods which will render comparable the work of different laboratories. At the session to-morrow morning preliminary reports from some of our sub-committees on these questions will be presented.

While the main object of the Association is to establish standard rules for testing, it is recognized that this cannot be done until a thorough knowledge is obtained of the properties of materials under varying conditions. Accordingly the work of some of the committees is to collect and digest the information now on record, or to make scientific investigations that will render present knowledge more complete and definite. Thus, there is a committee on the properties of steel at abnormally low temperatures, one on the relation of the chemical composition of stone to its weathering qualities, one to digest the work of previous conferences and conventions on the adhesion of hydraulic cement, one on the causes of the abnormal behavior of cements as to time of setting, and one on the protection of wood against the action of dry rot. Some of these subjects have already been discussed at the Congresses of Zurich and Stockholm, and accordingly the reports to be presented to the Paris Congress should contain positive additions to present knowledge.

At the annual meeting of this Section, held last year, the desire was expressed to discuss the subject of impact tests, and a special committee was appointed whose report will be presented at this meeting. Later, other members requested that other problems should be taken up by the Section, and accordingly three other American committees have been organized on special problems connected with the manufacture of iron and steel. While these committees have no connection with the international ones, it is believed that their work will add to the interest of our annual meetings, and further the general objects of the Association.

There are advantages and disadvantages in doing technical work by committees. One advantage accrues through the harmonization of the different views held by individuals, whereby non-essentials are rejected and only fundamental methods retained. One of the disadvantages is that this process of harmonizing views takes time, causing reports to be long delayed, particularly with international committees. Some technical societies appoint committees with great reluctance, fearing that their reports may be regarded as official action. In the case of our international organization, no such fear is felt, and the report of a committee is to be considered from the same point of view as the paper of an individual member. Through the formation of the national sections, the work of the international committees can certainly be made more valuable and effective than ever before, for each national subcommittee, after having eliminated disagreements of its individual members, can work as a body to impress its views upon the other national sub-committees. In many cases an international agreement may be found difficult to make, but if made after such full discussion it will be sure to be authoritative and valuable.

The subject of the chemical analysis of iron and steel has been discussed in previous conferences and congresses, and at the Stockholm meeting of 1897 it was for-

mally resolved to establish an international sidero-chemical laboratory at Zurich. It was stated that fifteen smelting companies and iron manufacturers had pledged themselves to contribute $3,500 per year for this purpose, and that the Polytechnicum at Zurich had offered the use of four well-equipped rooms. It was, accordingly, determined to open the laboratory in 1898, and an international commission was appointed to take charge of it and raise further funds for its maintenance. I am unable to state how fully this has been carried out, as no published accounts of its work have appeared. It is, however, to be doubted whether the establishment of chemical and physical laboratories falls properly within the scope of the objects of the Association. If sufficient funds could be raised so that men of different nationalities might meet at such a laboratory to actually make analyses and tests, each criticizing the others, while at the same time learning from them, then undoubtedly effective work would be done in harmonizing differences and perfecting standard methods. It is to be hoped, if the establishment of the sidero-chemical laboratory at Zurich proves to be successful, that it may tend to further this method of research. It is, however, the opinion of many members that results as good, if not better, would be secured by arranging systematic schemes of investigation and distributing the actual work of analysis or testing among the laboratories of different countries.

A brief history of the organization and work of the International Association for Testing Materials, and of that of its American Section, has now been given. The great interest taken in the movement in so many countries is an index of the necessity felt in all branches of the engineering profession for the introduction of uniform methods of testing and inspecting the materials of construction. This work is one that must occupy many years, and which in a certain sense can never be finished, for constant progress will be made in our knowledge of the properties of materials. In order to carry it on with success it is apparent that more money will be needed than the small amount now raised from the annual dues of members. In Europe the importance of the work of the Association is forcibly attested by the fact that engineering societies, bureaus of public works, iron and cement manufacturers and railroads assist it by extra annual contributions, and it is to be hoped that the influence of this Section may be sufficient to cause similar substantial gifts to flow into its treasury from American corporations.

Since the above was written a circular of the International Council has been received, containing the information that probably arrangements cannot be made for holding the Congress of the Association at Paris in 1900. It appears that the authorities of the Paris Exposition have the right to control the organization of all Congresses held in that city in that year, and that they have announced one to be held on the subject of materials, and appointed officers to conduct the same. The subject will be discussed at this annual meeting, and expressions of opinion are desired as to whether it is best to abandon our Congress of 1900, in order to coöperate with the one announced by the authorities of the Paris Exposition, or to hold it at London during the week preceding.

In conclusion, it is with pleasure that I congratulate the American Section upon its activity and the Association itself upon the bright prospects before it. The undertaking inaugurated by Bauschinger and his associates bore good fruit at the conventions of 1884, 1886, 1888 and 1893, and prepared the way for the Zurich meeting of 1895, which was at the same time the fifth convention and the first Congress. At the

Stockholm Congress of 1897 the true international work was begun, and the problems there proposed are now the subjects of careful study in all parts of the earth. Let us hope that the reports to be presented at the future Congresses will be such as to add to the present stock of knowledge, prove advantageous to both producers and consumers, and assist all engineers in economically using the materials and forces of nature for the benefit of man.

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*THE DEVONIAN SYSTEM IN CANADA.**

I.

To the student of the early literature of the Paleozoic rocks, and especially to the paleontologist, the name of William Lonsdale will always be associated with the Devonian System.

Although the term Devonian was first definitely proposed by Sedgwick and Murchison in a paper read April, 1839, and published in the fifth volume of the second series of Transactions of the Geological Society of London, the authors of this paper are careful to state (1) that "Mr. Lonsdale, after an extensive examination of the fossils of South Devon, had pronounced them, more than a year ago, to form a group intermediate between the Carboniferous and Silurian systems," and (2) that "the previous conclusions of Mr. Lonsdale * * * led the way to their proposed classification of the Cornish and Devonian formations."

Lonsdale, himself, in another paper printed in the same volume, distinctly claims that his suggestion, on the evidence of their fossils, that the South Devon limestones are "of an intermediate age between the Carboniferous and Silurian systems, and consequently of the age of the old red sandstone," was first made in December, 1837. S. P. Woodward, in the preface to the first part of his 'Manual of the Mollusca,' dated March, 1856, speaks of Lonsdale as his "friend and master, *the founder of the Devonian system in geology.*"

Yet so lately as in August, 1897, Mr. Marr is stated to have said* that "the Devonian system had been founded on stratigraphical grounds by Murchison and Sedgwick, and on paleontological grounds by Lonsdale and Etheridge." Surely it would have been more correct to have said that the existence of the Devonian as a distinct geological system was first indicated by Lonsdale in 1837 on purely paleontological evidence, and subsequently confirmed by Sedgwick and Murchison in 1839 on stratigraphical considerations.

However this may be, rocks of Devonian age have been discovered at various times in almost every province and district of the Dominion, and it is thought that a brief summary of the history of these discoveries and of the present state of our knowledge of the Devonian rocks of Canada, from a paleontologist's point of view, may be of interest on this occasion. In accordance with long usage in Canada, the line of demarcation between the Silurian and Devonian systems in this address will be drawn at the base of the Oriskany sandstone. It will also be convenient to consider the information that has so far been gained about the Devonian rocks of Canada in geographical order, from east to west, under the three following heads, viz.: (1) The Maritime Provinces and Quebec; (2) Ontario and Keewatin, and (3) Manitoba and the Northwest Territories.

I. THE MARITIME PROVINCES AND QUEBEC.

Nova Scotia.—In a memoir accompanying a geological map of Nova Scotia, by Dr.

* Address of the Vice-President and Chairman of Section E—Geology and Geography—of the American Association for the Advancement of Science, Columbus Meeting, August, 1899.

* *Quarterly Journal of the Geological Society of London*, Vol. LIII., page 460.

Abraham Gesner, published in the Proceedings of the Geological Society of London for May 10, 1843,* the following paragraph occurs:

"*Old Red Sandstone or Devonian group.*—Above the Silurian beds there occurs in several parts of the province, a bright red micaceous sandstone or conglomerate, accompanied by thin beds of red shale and marly clay, and in some places containing seams of fibrous gypsum. Hitherto no organic remains have been found in it. At Advocate Harbor and on the Moose River this sandstone is seen lying uncomformably beneath the coal measures. At the latter locality the sandstone dips W. 21° and the coal measures dip N. N. E. 60°. It is from a joint consideration of the mineral characters of this formation, and its relative position as compared with the coal measures, that the author has regarded it as the equivalent to the old red sandstone."

This would seem to be the earliest statement in regard to the occurrence of rocks of Devonian age in British North America, but Gesner then included in his old red sandstone group certain outliers of Carboniferous limestone and possibly Trias, that are now known to be associated with rocks still held to be Devonian.

Not quite two years later than this, in a paper read before the Geological Society of London on January 22, 1845, Sir William Dawson says that beyond Cape John the newer coal formation "seems to overlie, unconformably, a series of hard grits, slates and limestones, with scales of *Holoptychius*, Encrinites and fragments of bivalve shells, and which are probably of newer Silurian or Devonian age. The last-mentioned rocks, with various kinds of trap, form an elevated ridge belonging to the Cobequid chain of hills." †

Influenced, as he elsewhere tells us, by information supplied by Sir Charles Lyell, Gesner's earlier statements as to the Devonian rocks of Nova Scotia were modified in his 'Industrial Resources of Nova Scotia,' published at Halifax in 1849. In this volume the paragraph about the Devonian rocks is as follows:

"*Old Red Sandstone or Devonian Group.*—Above the Silurian strata there occur thick beds of conglomerate, bright red and micaceous sandstones, red shale and marly clay. At Advocate Harbor, Parrsboro,' Moose River, Horton, Shubenacadie and other places these rocks are seen dipping beneath the coal measures and gypsiferous red sandstones. The scales of fishes and other organic remains found in these deposits are too scanty and imperfect to afford conclusive evidence of their relative age; but from a joint consideration of them, the mineral character of the formation and its position, it may be classed as the equivalent of the old red sandstone of Europe or a part of the great carboniferous series. The strata contains but few minerals of importance."

The first edition of the 'Acadian Geology,' by Sir William Dawson, published in 1855, contains a 'Tabular View of Rock Formations in Nova Scotia,' in which the Devonian is defined as including the "fossiliferous slates of Bear River, Nictaux, New Canaan, Pictou, Arisaig, etc., and perhaps also parts of the metamorphic rocks of the Cobequid and Pictou, hills." In the fourteenth chapter of this volume the fossiliferous slates at Arisaig and the East River of Pictou are regarded as of Devonian age, on the authority of James Hall, but in a supplementary chapter, dated August, 1860, they are referred to the Silurian. Nine 'fossils from the Devonian and upper Silurian (?) rocks of Nova Scotia' are figured in this volume, but none of these are specifically determined and only three are Devonian. But, in the supplementary chapter, four of the fossils of the Nictaux and one of the Bear River series are determined specifically. Of the former it is stated that Hall "compares them with the fauna of the Oriskany sandstone, and they seem to give indubitable testimony that the Nictaux iron ore is of Lower Devonian age." A fuller list of fossils from Bear River and Nictaux, in which sixteen species

* Vol. IV., Part I., p. 187.

† *Quarterly Journal of the Geological Society of London*, Vol I., p. 235.

are described generically and nine specifically, was published in 1891.*

In the second and much enlarged edition of the 'Acadian Geology,' published in 1868, Sir William Dawson confirms and elaborates most of the statements about the Devonian of Nova Scotia in the first edition and 'Supplementary Chapter,' and figures a new Devonian Spirifer (*S. Nictavensis*) from Nictaux.† He notes the occurrence of 'obscure remains, evidently of land plants,' in more or less altered rocks on the flanks of the Cobequids, etc., and more particularly the discovery, in 1866, of "stipes of ferns, apparently of two species, a *Pinnularia*, and branching stems much resembling those of *Psilophyton*, a characteristic Devonian genus," in a gray altered sandstone or quartzite underlying unconformably a Carboniferous conglomerate at Bear Brook (now known as McCulloch Brook), near the Middle River of Pictou.

Doctor Honeyman, in a paper read before the Nova Scotia Institute of Natural Science in November, 1870, and since published in its Transactions, describes as of Devonian age a red band of argillites on McAra's and McAdams' Brooks, near Arisaig, which he calls the 'McAra's Brook Strata,' but in which he did not succeed in finding any fossils. Later collectors, however, have been more successful, and in 1885, Mr. T. C. Weston, of the Canadian Geological Survey, obtained from these argillites "fragments of plants and fish teeth not certainly determinable, together with certain interesting" imprints "like those of *Protichnites carbonarius*."‡ From the same rocks, in 1897, Dr. Ami and Mr. Hugh Fletcher, of the same survey, collected fragments of *Pterygotus* and of Pteraspidian and other fishes. The fish remains obtained in these rocks in 1897 have been examined by Mr. A. Smith Woodward, of the British Museum, who thinks that they are either uppermost Silurian or lowermost Devonian.

From 1872 to the present time Mr. Fletcher has been engaged in a minutely detailed examination of the geological structure of northern and eastern Nova Scotia, for the Geological Survey of Canada, which has published geological maps of a greater portion of this area on a scale of one mile to the inch. In 1887 he referred to the Devonian system the rocks below the Carboniferous conglomerate at Loch Lomond, Richmond County, Cape Breton.* From that point he has since traced rocks that he has described as Devonian, on stratigraphical and lithological grounds, westward through the peninsula of Nova Scotia as far as the head of Cobequid Bay and along both sides of Minas Basin, where he has estimated that they attain a thickness of from 10,000 to 15,000 feet.† With some Silurian and the associated igneous rocks, he believes them to form the mass of the Cobequids.

Most of these rocks that Mr. Fletcher refers to the Devonian had, however, previously been referred to other geological horizons. Among the more notable of these are the Horton series in Kings County, and the Riversdale series and Harrington River rocks in Colchester County. On purely paleontological evidence the Horton series had been referred to the Lower Carboniferous, and the Riversdale series to the Millstone Grit, by Sir William Dawson, though it is now pretty generally conceded that both are unconformably overlaid by a marine Carboniferous limestone.

* Acadian Geology, Supplementary Note to the Fourth Edition, pp. 20 and 21.

† Page 499, Figs. 176, *a*, *b*.

‡ Geological and Natural History Survey of Canada, Annual Report, New Series, Vol. II., p. 68 P.

* Geological Survey of Canada, Report of Progress for 1877–78.

† See the Annual Reports of the same Survey for 1877–78, 1879–80–81, 1886, and 1890–91.

Owing to circumstances it has unfortunately happened that very little paleontological work has been done in Nova Scotia or on Nova Scotian material since 1873. With the view of stimulating the prosecution of researches in this direction, collections of fossils have been made, during the past four years, and chiefly by Dr. Ami, of the Geological Survey of Canada, from many localities in the province, and some selected sets of these fossils have been forwarded to specialists.

In the Christmas and New Year's week of 1897 and 1898 Mr. David White, of the United States Geological Survey, examined the fossil plants from Nova Scotia and New Brunswick in the Peter Redpath Museum at Montreal and in the Museum of the Geological Survey at Ottawa. On the evidence of these plant remains Mr. White came to the following conclusions, which are summarized, by permission, from an unpublished report, in the form of a letter addressed to Dr. H. M. Ami, and dated January 12, 1898: (1) That the plant-bearing portion of the Horton series of Nova Scotia, as shown by Sir William Dawson in 1873, is nearly contemporaneous with the Pocono formation of the eastern United States, which has long been assigned to a basal position in the Carboniferous system. (2) That the Riversdale series of Nova Scotia (which Sir William Dawson referred to the Millstone Grit) is of Carboniferous age and assuredly newer than the Horton series. (3) That the plant-bearing beds near St. John, New Brunswick, are not Middle Devonian, as had previously been supposed, but Carboniferous, and that they are the exact equivalents of the Riversdale series of Nova Scotia.

Early in January last, collections of fossil plants from the Horton and Riversdale series and Harrington River rocks, at several localities in Nova Scotia, were sent to Mr. R. Kidston, of Stirling, Scotland, an experienced paleo-botanist, for examination and study. In a manuscript report upon these collections, addressed to the Director of the Canadian Survey, and received May 8, 1899, Mr. Kidston comes to almost exactly the same conclusions as those previously arrived at by Mr. White, and on perfectly independent grounds. In this report Mr. Kidston expresses the following opinions: (1) Of the Horton series he says: "These rocks appear to be undoubtedly Lower Carboniferous." "There is no evidence at all to support the opinion that they are of Devonian age." "All the evidence derived from a study of their fossils points very strongly against this view." (2) Of the Riversdale series he says: "The two divisions of this series, the Riversdale Station and Harrington River rocks, may be treated together, as they contain the same fossils and are evidently of the same age." The whole of the fossil plants from the Riversdale series have a most pronounced Upper Carboniferous facies and markedly possess the characteristics of a Coal Measure Flora. "Judged from a European comparison, no other conclusion can be arrived at." (3) Lastly, he says that "the question of the age of the Riversdale series is inseparably connected with the question of the age of the plant beds of St. John, New Brunswick." "The species contained in the Riversdale series are also met with in the St. John plant beds, where, however, a greater number of species has been discovered." "I do not," he adds, "wish to express my views as to the age of the St. John plant beds too strongly, but, from what I have been able to learn from a study of the literature of the subject and an examination of specimens from these beds, it appears to me that they possess a flora of a much higher horizon than that assigned to them, and that in reality they are most probably Upper Carboniferous." "It must, however, be remembered that since Sir William Dawson

wrote his work on the Pre-Carboniferous Flora very much has been done in Europe to work out the zones of the Coal Measure Flora, and careful and accurate figures have been published which did not exist at the time he was carrying out his investigations." "A thorough revision of the work, especially in the light of subsequent collections and possible discovery of more perfectly preserved specimens, seems most desirable, and also that a better series of figures be published."

As complete a collection as possible of the fish remains of the Horton and Riversdale series of Nova Scotia was sent to Mr. A. Smith Woodward, in January, 1899, for examination and study, but no report upon these specimens has yet been received.

The Devono-Carboniferous problem in Nova Scotia and New Brunswick is far too complicated a question to be discussed at any length in an address of this kind. At present, however, it is obvious that there is some discrepancy between the views of the two geologists on the Canadian Survey staff, who have studied the question from a stratigraphical and lithological point of view, and those of the paleontologists whose names have been cited in this connection, as to the age of the Horton and Riversdale series of Nova Scotia, and of the plant-bearing beds near St. John, New Brunswick.

New Brunswick.—It would appear that Devonian rocks, or at any rate rocks that have for many years been regarded as of Devonian age, were not recognized in New Brunswick until 1861. For, although Dr. Gesner made extensive geological explorations in the province last named, from 1838 to 1843, the strata that he refers to the old red sandstone, in his first report on a geological survey thereof, published in 1839, and in a short paragraph in chapter eleven of his volume on New Brunswick, published in 1847, are now regarded as of Carboniferous age.

The occurrence of fossil plants in rocks near St. John was noticed by Dr. Gesner as early as in his second report on the Geology of New Brunswick, published in 1840, and Sir William Dawson states that a well-characterized specimen from these rocks, which he subsequently identified with the *Calamites transitionis* of Goeppert, was shown to him by the late Professor Robb in 1857.*

In 1860 a small collection of fossil plants from the shales at the foot of the city of St. John, near the barracks, recently made by Dr. G. F. Matthew, was submitted to Sir William Dawson for examination. On the evidence of their fossil plants these rocks at St. John were referred to the Devonian system by Sir William, in a paper 'On the Pre-Carboniferous Flora of New Brunswick, Maine and Eastern Canada,' published in the *Canadian Naturalist and Geologist* for June, 1861. Seven species are recognized in this collection, six of which are described as new. Professor L. W. Bailey, in his Report on the Geology of Southern New Brunswick, says that "the same author in June, 1861, after an examination of certain fossils in eastern Maine, asserted the Devonian age of the rocks containing them, and also of the sandstones constituting the peninsula of St. Andrews, which they closely resemble."

Immediately after this, rocks containing similar fossils, and presumably, therefore, of Devonian age, were recognized at other localities in the neighborhood of St. John, or in St. John county, as at the Little and Mispec rivers, and more particularly at the Fern Ledges, in Lancaster parish. From the latter locality extensive collections oi fossils were made by Dr. Matthew, Professor Hartt and other local collectors in 1861, 1862 and 1863, and more recently by Mr. W. J. Wilson and Dr. Matthew. The luxuriant and singularly varied fossil flora of the Fern Ledges has been described by

* Acadian Geology, Second Edition, p. 502.

Sir William Dawson in 1862,* by Professor Hartt in 1865,† by Sir William Dawson and Professor Hartt in 1868,‡ and by Sir William Dawson in 1871§ and 1882.|| The 'revised list of the Pre-Carboniferous plants of N. E. America' in the first part of Sir William's memoir on 'the fossil plants of the Devonian and Upper Silurian formations of Canada,' published by the Dominion Survey in 1871, contains the names of seventy species of fossil plants from the Devonian of New Brunswick, nearly all of which are from the Fern Ledges. In the second part of the same memoir, published in 1882, two additional species were described.

The remarkable assemblage of air-breathing articulata and mollusca associated with these plant remains has been described by Salter in 1863,¶ by Scudder in 1868,** by Sir William Dawson in 1880,†† and by Dr. Matthew in 1888‡‡ and 1894.§§. In the latter of these two papers Dr. Matthew states that the "air-breathing articulates of the plant-bearing bed of St. John so far recognized consist of:

Insects, nine species of eight genera 9
Myriapods, six species of several genera......6
Arachnid similar to Anthracomartus..........1
Probable pedipalp. (*Eurypterella*)........... 1
Probably Arachnid or Isopod (*Amphipeltis*)....1
Scorpion (*Palæophonus arctus*)................1

"Two species of land snails have also been found, raising the number of air-breathing animals found in the plant-beds of St. John to twenty-one kinds."

Elsewhere in this paper Dr. Matthew says that "later discoveries lead the author to think that *Eurypterus pulicaris*, Salter, should be referred to the myriapods or to the insects," and in the foregoing list it is evidently included with the insects. To this list, also, should be added a trilobite and an annelid (*Spirorbis Erianus*, Dawson), which indicate marine or at least brackish water conditions, while from the description and figures it is difficult to see in what respects the very imperfect specimen described as a land shell under the name *Strophites* (since changed to *Strophella*) *grandæva* differs from the presumably marine genus *Macrocheilus*.

Detailed description of the stratigraphical relations of the presumed Devonian rocks near St. John, by Dr. Matthew, were published in 1863* and 1865,† and many additional facts in relation thereto are contained in Professor Bailey's Report on the Geology of Southern New Brunswick, published in 1865. In 1863 Dr. Matthew gave the local and provisional names of the Mispec, Little River and Bloomsbury groups to the subdivisions of the supposed Devonian system in St. John county, the Little River group, including both the Cordaites shales of the Fern Ledges, with their numerous fossil plants, insects, etc., and the Dadoxylon sandstone. The Little River group was at first supposed to be of Upper Devonian age; but, in consequence of the investigations of Professor Bailey and Dr. Matthew in 1870, Sir William Dawson, in 1871, expressed the opinion that the Mispec group represents the Upper Devonian, the Little River group the Middle Devonian, and the Lower Conglomerates (presumably the

* *Quarterly Journal of the Geological Society of London*, Vol. XVIII., pp. 296-330.

† In an Appendix to Professor Bailey's Report on the Geology of Southern New Brunswick.

‡ Acadian Geology, Second Edition, pp. 534-556.

§ Geological Survey of Canada. Fossil Plants of the Devonian and Upper Silurian Formations of Canada.

|| *Ibid.*, Part 2.

¶ *Quarterly Journal of the Geological Society of London*, Vol. XIX., pp. 75-80.

** Acadian Geology, Second Edition, pp. 523-526.

†† *American Journal of Science*, Vol. XX., p. 413.

‡‡ Transactions of the Royal Society of Canada, Vol. VI., Sec. 4, pp. 57-62.

§§ *Ibid.*, Vol. XII., Sec. 4, pp. 95-100.

* *Canadian Naturalist and Geologist*, Vol. VIII., pp. 241-259.

† *Quarterly Journal of the Geological Society of London*, Vol. XXI., pp. 429-30.

Bloomsbury group) the Lower Devonian. Matthew, in 1888, after stating that there is one unconformity between the Perry sandstone and the Mispec beds and another between the Mispec beds and the Cordaite shales, thus redivides the Devonian rocks of St. John county, the unconformities being marked by a dividing line.

"Perry Sandstones with Upper Devonian flora, according to Sir J. W. Dawson, but lithologically representing the Lower Carboniferous sandstone.

"Mispec Conglomerate and slate.

"Cordaite shales and flags, Middle Devonian flora. *Insect remains* (in oldest beds of the Cordaite shales).

"Dadoxylon sandstone (with an older Devonian flora, G. F. M.).

"Bloomsbury Conglomerate, etc."*

On behalf of the Canadian Survey, in 1870, Professor Bailey and Dr. Matthew traced beds corresponding to the plant-bearing beds near St. John as far to the westward as Lepreau Harbor, in Charlotte county, where many fossil plants like those at the Fern Ledges were collected. Ten years later the distribution of the Devonian rocks in the southern part of the province, as far as then known, was thus summarized by Messrs. Bailey, Matthew and Ells :

"The areas of Devonian occurring in southern New Brunswick may be stated as follows :

"1. A large basin, or double synclinal, east of St. John Harbor, occupying the valley of the Mispec, with a southern area extending northeasterly across the Black River, near the forks of the East Branch.

"2. Isolated outcrops on Coal Creek and on Canaan River and North Fork, presumably of this age, but lacking evidence of fossils.

"3. Small areas about St. John and Carlton, with possibly Partridge Island.

"4. A small area about the eastern extremity of Spruce Lake, on the St. Andrews Railroad.

"5. A belt stretching west from Musquash Harbor to Lepreau Harbor, in which is contained the so-called anthracite mine of Belas Basin, with a smaller detached area along the shore from By Chance Harbor to Dipper Harbor.

"6. A large area in the northern part of Charlotte county, embracing the former pale argillite series and extending into Queen's county."*

Prior to 1894 the Devonian age of these rocks had never been called in question. But, in a footnote to page 79 of Sir William Dawson's 'Synopsis of the Air-Breathing Animals of the Palæozoic Period in Canada up to 1894,' published in the Transactions of the Royal Society of Canada for that year, Dr. Matthew says of the Little River group (which includes the plant-bearing beds near St. John) that he has "recently found some reason to suspect that these beds are as old as Silurian." And, as already stated in connection with this phase of the Devono-Carboniferous problem in Nova Scotia, both Mr. White and Mr. Kidston, on the evidence of their plant remains, have independently and quite recently expressed the opinion that the plant-bearing beds near St. John are the exact equivalents of the Riversdale series of the Nova Scotia Carboniferous.

In northern New Brunswick an area of gray shale (with *Psilophyton*) and conglomerates, which are regarded as of Devonian age, on the east side of the St. John River, near the mouth of the Beccaguimic, is indicated in a map accompanying Dr. Ells' 'Report on the Iron-ore Deposits of Carleton County,' in the 'Report of Progress of the Geological Survey of Canada for 1874–75.' Dr. Ells, also, in the 'Report of Progress' of the same Survey for 1879–80, says that areas of Devonian rocks are "seen at intervals along the lower Restigouche River," and that they "form a synclinal basin extending from near the town of Dalhousie westward to a point about two miles above Campbellton and terminating on the south side of the river at Old Mission Point." This report is descriptive of explorations made in 1879, and in it the Devonian age of

* Transactions of the Royal Society of Canada, Vol. VI., Sec. 4, p. 61.

* Geological Survey of Canada, Report of Progress for 1878–79, p. 11 D.

the rocks at Campbellton is assumed exclusively on the evidence of a few fossil plants (*i. e.*, two species of *Psilophyton*, one of *Lycopodites* and one of *Cordaites*) that had been identified and described by Sir William Dawson. The remarkable fish-fauna at Campbellton was not discovered until June 27, 1881, but it will be more convenient to consider it later on, in connection with the equally notable fish-fauna discovered in 1879, on the opposite side of the lower Restigouche River at Scaumenac Bay, in the Province of Quebec, as the two localities are only about sixteen miles apart. Another area of Devonian rocks in the northern part of the province is that on the Upsalquitch River, discovered by Dr. Ells in 1879 and described also in the 1879–80 report.

Quebec.—The Geological Survey of Canada was instituted in August, 1842, but prior to the confederation of the provinces in 1867 the scope of its operations extended only over Upper and Lower Canada, now known as the Provinces of Ontario and Quebec.

With the view of ascertaining whether the coal measures of New Brunswick did or did not extend into Canada, its first Director, Sir W. E. Logan, devoted the summer seasons of 1843 and 1844 to a geological examination of the Gaspé peninsula and of the country between it and the Baie des Chaleurs. In 1843 he surveyed the coast from Cap Rosier to Paspebiac, and in 1844 the exposures between Cap Rosier and Cape Chatte, thence following the Chatte River to the Cascapedia and crossing to the Baie des Chaleurs. During these two years the main geological features of the part of the province examined were, for the first time, definitely ascertained, and the absence of any productive coal measures north of the Baie des Chaleurs demonstrated. In 1843 the sandstones and limestones of Gaspé Bay, since known as the Gaspé sandstones and limestones, were carefully studied and their fossils collected. In 1844 the Gaspé sandstones were traced for a considerable distance up the St. Lawrence, and in the 'Report of Progress' of the Survey for 1847–48 they are said to extend from the very extremity of the Gaspé district to Matapedia Lake, a distance of 150 miles, and their thickness is estimated at 7,000 feet.

As early as 1845, if not in 1844, the Devonian age of the Gaspé sandstones was recognized by Logan. In the Annual Report of the Survey, under his direction for 1844 (which, though written in 1845, was not published until 1846), these sandstones are said to "resemble the Chemung and Portage groups of the State of New York, with perhaps the addition of what the geologists of that State term their old red sandstone" (*i. e.*, the Catskill group), and to be overlaid by the Carboniferous series. At that time the Gaspé sandstones were regarded as of Upper Devonian age, but the numerous fossils that Logan had collected from them had not then been critically studied by any competent paleontologist. In an entry in his notebook for August 20, 1843, published in the 'Life of Logan' by Dr. Harrington, it is distinctly stated that the plants of these sandstones are 'not Carboniferous.' A few years later, in a communication to the meeting of the 'British Association for the Advancement of Science' at Ipswich, in 1851, Logan thus expresses himself: "None of the productive part of the New Brunswick coal measures reaches Canada, but there comes out from beneath it, on the Canadian side of the Bay Chaleurs, 3,000 feet of Carboniferous red sandstones and conglomerates. These are succeeded by 7,000 feet of Devonian sandstones, which rest upon 2,000 feet of Silurian rocks consisting of limestones and slates."*

Six of the species of fossil plants collected from the Gaspé sandstones by Logan in

* Report of the Twenty-first Meeting, page 61.

1843 were described by Sir William Dawson: four (*Psilophyton princeps*, *P. robustius*, *Lepidodendron Gaspianum* and *Prototaxites Logani*) in the *Quarterly Journal of the Geological Society of London* for January, 1859;* and two (*Cordaites angustifolia* and *Selaginites formosus*) in the *Canadian Naturalist and Geologist* for June, 1861. In the former of these papers the two remarkable genera *Psilophyton* and *Prototaxites* were first proposed and defined. Subsequently, however, in 1888, Sir William somewhat modified his earlier descriptions of *Prototaxites* and changed its generic name to *Nematophyton*.† *Selaginites formosus* was abandoned 'as a vegetable species' by its author in 1871, because additional material showed that the specimens upon which it was based are 'probably fragments of some Eurypteroid crustacean,'‡ as suggested by Mr. Salter.

The supposed worm-tracks from the Gaspé sandstone between Tar Point and Douglastown, discovered by Logan in 1843, were described and refigured by the writer, under the name of *Gyrichnites Gaspensis*, in the Transactions of the Royal Society of Canada for 1882.

Logan's examinations of the Gaspé series of sandstones and limestones were supplemented by those of Murray on the Douglastown and St. John rivers in 1845; of Richardson on the Magdalen River and upper part of the Dartmouth in 1857, and of Bell of the Dartmouth, York and Malbaie rivers in 1862. Sir William Dawson also made extensive collections of fossils around the shores of Gaspé Bay in 1858 and 1869, and Dr. Ells a general geological survey of the Gaspé peninsula, from Gaspé Basin to the Matapedia River and from the St. Lawrence River to the Baie des Chaleurs in 1880–83, and a similar survey of the Devonian basin of the Causupscal River in 1884.

The collections made by Sir William Dawson in 1869 added thirteen additional species of fossil plants to the flora of the Gaspé sandstones, and these species were described and illustrated in the first part of his memoir on the 'Fossil Plants of the Devonian and Silurian Formations of Canada,' published by the Canadian Survey in 1871. The 'Geology of Canada,' published in 1863, contains lists of some of the marine invertebrate fossils of the Gaspé limestones and sandstones, collected by Logan, Dawson and Bell, and these fossils were more fully determined or described by E. Billings in the first part of the second volume of Paleozoic Fossils, published by the Canadian Survey in 1874. A small species of *Cephalaspis*, also, collected by Professor G. T. Kennedy, then one of Sir William Dawson's assistants, from the Gaspé sandstones on the north side of Gaspé Bay in 1869, was described and printed by Professor Ray Lankester, in the *Geological Magazine* for September, 1870,* under the name of *C. Dawsoni*.

In the 'Geology of Canada' it is stated that the "limestones of Cape Gaspé appear for the most part to belong to the Lower Helderberg group." * * * "The fossils at the summit, however, bear a striking resemblance to those of the Oriskany formation, with which several of them are identical. It appears probable, therefore, that we have a passage from the Lower Helderberg to here the Oriskany, and the latter formation may be more especially represented by the lower part of the Gaspé sandstones." Eleven years later, in 1874,† E. Billings expressed the opinion that the lower 330 feet of the Gaspé

* Vol. XV., p. 477.

† The Geological History of Plants, page 21; and Transactions of the Royal Society of Canada for 1888, Sec. 4, pp. 27–47.

‡ Geological Survey of Canada. The Fossil Plants of the Devonian and Upper Silurian formations of Canada, Part 1, p. 65.

*Volume VII., p. 397.

† Geological Survey of Canada. Paleozoic Fossils, Vol. II., Pt. 1, p. 1.

limestones are Upper Silurian (Lower Helderberg), the middle 880 feet passage beds, and the upper 800 feet Devonian.

At the other end of the province a small area of rocks on the Famine River, in Beauce county, and another on the west side of Lake Memphremagog, in the county of Brome, were recognized as Devonian by Logan in 1863.*

Quite recently, too, a re-examination, by Mr. Schuchert, of some of the brachiopoda from the small masses of limestone on St. Helen's Island, opposite Montreal, has shown that these limestones are probably the equivalents of part of the Hamilton formation of Ontario and New York, and not of the Lower Helderberg.

Although the Devonian system is preeminently the Age of Fishes, yet for many years scarcely any remains of fossil fishes had been found in the Devonian rocks of Canada that are at all closely comparable with those of the old red sandstone of Scotland and Russia. As early as 1842, however, the rocks on both sides of the lower Restigouche River were examined by Dr. Gesner, who says that he found the "remains of fish and a small species of tortoise, with fossil foot-marks,"† in the shales and sandstones at Escuminac (now called Scaumenac) Bay, which he supposed were of Carboniferous age. The statement in regard to the fossils at this locality attracted no particular attention at the time, but in September, 1879, Dr. Ells found a natural mould of the exterior of the ventral surface and of one of the lateral appendages of a Pterichthys-like fish in a concretionary nodule at Scaumenac Bay, and in June, 1881, remains of a species of *Cephalaspis* in the brecciated limestones near Campbellton. The first of these discoveries led to further investigations by officers of the Canadian Survey in 1880, 1881 and 1882, which revealed the existence of a remarkable assemblage of fossil fishes and land plants of Upper Devonian age at Scaumenac Bay, and of an entirely different series of fishes and plants of Lower Devonian age on the opposite, or New Brunswick side of the river, near Campbellton. Large collections were made at each of these localities, especially of the fossil fishes, which were described by the writer in 1880,* 1881† and 1883,‡ and described and illustrated in 1887§ and 1889.|| Many of these specimens were exhibited at the meeting of this Association at Montreal in 1882.

In the collections from Scaumenac Bay made up to 1882 and described by the writer the Elasmobranchii are represented by two species of *Acanthodes* (*A. concinnus* and *A. affinis*); the Ostracodermi by numerous, remarkably well-preserved and nearly perfect specimens of a *Bothriolepis* (*B. Canadensis*) which Gesner seems to have thought was a small tortoise; the Dipnoi by a supposed *Phaneropleuron* (*P. curtum*), the type of Traquair's subsequently described genus *Scaumenacia*,¶ and the Teleostomi by a *Glyptolepis* (*G. Quebecensis*), a *Cheirolepis* and a new genus (*Eusthenopteron*) closely allied to *Tristichopterus*. A few of the superficial and presumably sensory grooves on the cranial shield of the Canadian *Bothriolepis* were mistaken for sutures, as the similar ones of the European species had been by Lahusen, but

* Geology of Canada, pp. 428–436.

† Report on the Geological Survey of the Province of New Brunswick, etc., St. John, 1843, p. 64.

* *American Journal of Science*, Vol. XX., p. 132; and reprinted in the *Canadian Naturalist and Geologist*, Vol. X., p. 23.

† *American Journal of Science*, Vol. XXI., p. 94; and reprinted in the *Annals and Magazine of Natural History*, Fifth Series, Vol. VIII., p. 159; and *Canadian Naturalist and Geologist*, Vol. X., New Series, p. 27 and p. 93.

‡ *American Naturalist*, Vol. XVII., p. 158.

§ Transactions of the Royal Society of Canada, Vol. IV., Sec. 4, p. 101.

|| *Ibid.*, Vol. VI., Sec. 4, p. 77.

¶ *Geological Magazine*, June, 1893, Decade 3, Vol. X., p. 262.

some of the specimens of that genus from Scaumenac Bay threw quite a new light on the structure of its mouth organs and of the so-called 'lid' with its pineal element. And, similarly, a portion of one side of the head of a specimen of *Eusthenopteron* from the same locality, which by an oversight was referred to *Phaneropleuron*, has almost all the sclerotic plates of the eye preserved.

From the collections made near Campbellton in 1881 and 1882 four species of fossil fishes were described, viz.: *Cephalaspis Campbelltonensis;* a supposed *Coccosteus* (*C. Acadicus*), the type of Traquair's subsequently characterized genus *Phlyctænaspis*,* and two kinds of fin spines.

Numerous fossil fishes from both of these localities have since been collected by Mr. Jex for Mr. R. F. Damon, of Weymouth, England, and these have been acquired by the Edinburgh and British Museums. These later collections have yielded some additional species, one from Scaumenac Bay, which was described by Dr. Traquair in 1890, and six from Campbellton, three of which were described by Dr. Traquair, one in 1890 and two in 1893, and three by Mr. A. Smith Woodward in 1892. The latest novelty from Scaumenac Bay is a new *Cephalaspis* (*C. laticeps*, Traquair), of which it is said that "this is the first occurrence of a cephalaspid in rocks of later age than the Lower Devonian."† The three additional species from Campbellton that Dr. Traquair has described are two ichthyodorulites (*Gyracanthus incurvus*‡ and *Cheiracanthus costellatus*)§ and another *Cephalaspis* (*C. Jexi*).|| The three from the same locality described by Mr. A. Smith Woodward, in the eighth volume of the Third Decade of the *Geological Magazine*, are all

* *Geological Magazine*, Decade 3, Vol. VII., p. 144.
† *Ibid.*, Decade 3, Vol. VII., p. 16.
‡ *Ibid.*, p. 21.
§ *Ibid.*, Decade 3, Vol. X., p. 146.
|| *Ibid.*, p. 147.

elasmobranchs, viz., *Acanthodes semistriatus*, *Protodus Jexi* and *Diplodus problematicus*, the latter being the type of Traquair's genus *Doliodus*,* published in 1893.

In 1882 Sir William Dawson determined or described the fossil plants from Scaumenac Bay, four specifically and four only generically, and identified six species of fossil plants from near Campbellton with the *Psilophyton princeps*, *P. robustius*, *Arthrostigma gracile*, *Leptophlœum rhombicum*, *Cordaites angustifolia* and *Prototaxites Logani* of the Gaspé sandstones. He asserts that the plant and fish-bearing beds at Scaumenac Bay are "no doubt the equivalents and continuation of the upper part of the Gaspé sandstones," and that the fossil plants from near Campbelltown are "perfectly identical with the lower part," of these sandstones.†

J. F. Whiteaves.
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(*To be concluded.*)

* *Ibid.*, p. 145.
† Geological Survey of Canada. The Fossil Plants of the Erian (Devonian) and Upper Silurian Formations of Canada. Part 2.

THE FAMILY NAME OF THE DORMICE.

In a paper 'On the Genera of Rodents,' published in 1896,‡ Mr. Oldfield Thomas very properly rejected the family name Myoxidæ commonly applied to the Old World dormice, for the reason that *Myoxus*, on which it was based, is a synonym of the earlier generic name *Glis*. In adopting the name Gliridæ he divided the family into two subfamilies, Glirinæ and Platacanthomyinæ; the former including four genera: *Glis* Brisson, 1762; *Muscardinus* Kaup, 1829; *Eliomys* Wagner, 1843, and *Graphiurus* Cuvier, 1838; the latter *Platacanthomys* Blyth, 1859, and *Typhlomys* Milne-Edwards, 1877. It now appears that Gliridæ is untenable for this family because of previous application to other groups; in

‡ Proc. Zool. Soc. London, 1896, p. 1016.

short, that it is one of the rare examples of a term preoccupied as a family name but based on a genus which is perfectly valid.

Nearly eighty years ago Wiegmann* proposed the family Glirina for the wombats (*Phascolomys*) of Australia and Tasmania, which are now referred to the Phascolomyidæ. This course was also followed by Wagner in 1855, in the Supplement to Schreber's Säugthiere (Vol. V., pp. xv, 332).

In 1837 Ogilby,† in discussing the relationships of the peculiar aye-aye (*Cheiromys*) found in Madagascar, remarked: "It is only, indeed, the absence of the marsupial character which would make us hesitate to unite the *Cheiromys* with the Didelphidæ; but this circumstance is so material as to require that it should be placed in a different subfamily. At the same time, its analogy to the Rodentia ought not to be overlooked; and it is for the purpose of expressing this relation that I propose to denominate the small group which I am obliged to form for this animal, Gliridæ. I suspect, indeed, that the *Cheiromys* bears a more intimate relation to the real dormice (*Glis*) than we are yet aware of."

Thus Gliridæ has been used for three different groups of mammals belonging to as many different orders, Marsupialia, Primates and Rodentia. But since it is generally recognized that family names must be based on one of the included genera, this name is not available either for the aye-aye or the wombat, while its prior application to these animals invalidates its later use for the dormice, the only group which contains a genus *Glis*.

It remains to be determined what family designation should be applied to the dormice. Besides *Glis* and its synonym *Myoxus*, two other genera have been selected as types of higher groups: *Platacanthomys*, made the type of the subfamily Platacanthomyinæ by Blyth in 1876, and *Graphiurus*, the type of the subfamily Graphiurini by Winge in 1887. Either of these names might be used for the family. Platacanthomyinæ has the advantage of priority, but is open to the objection that it represents an aberrant section, so different, in fact, that some authors have not associated it with the dormice at all. *Graphiurus* is also aberrant, and according to Winge should be separated from all the other genera. This view Thomas does not accept, holding that "it might be quite as correct to separate *Glis* and *Muscardinus* on the one side from *Eliomys* and *Graphiurus* on the other by the pattern of the teeth, as to separate the last-named from the rest by the structure of the anteorbital region." Evidently a family based on the Indian *Platacanthomys* or the South African *Graphiurus* would not represent exactly the same group as that formerly known as Myoxidæ.

Under these circumstances it seems desirable to adopt a new family name, Muscardinidæ, based on *Muscardinus*, a genus which is closely related to *Glis*. The family of dormice may then be subdivided into the Muscardininæ for *Muscardinus, Glis, Graphiurus, Eliomys;* and Platacanthomyinæ for *Platacanthomys* and *Typhlomys*, reserving Winge's Graphiurinæ for *Graphirus* and *Eliomys*, in case it should be desirable to make a third subfamily for these genera, as suggested by Thomas.

T. S. PALMER.

WASHINGTON, D. C.

* Wiegmann & Ruthe's Handbuch d. Zool., p. 52, 1832.

† *Charlesworth's Mag. Nat. Hist.*, I., p. 523, Oct., 1837.

SCIENTIFIC BOOKS.

Differential and Integral Calculus for Technical Schools and Colleges. By P. A. LAMBERT, M.A., Assistant Professor of Mathematics, Lehigh University. New York, The Macmillan Company. 1898. Pp. x+245. Price $1.50.

An Elementary Course in the Integral Calculus. By DANIEL ALEXANDER MURRAY, Ph.D., Instructor in Mathematics in Cornell University. New York, Cincinnati, Chicago, The American Book Company. 1898. Pp. xiv + 288. Price.

The late Judge Cooley, it is said, made it a point to advise his students never to buy a law book not containing a suitable index. If Professor Lambert's book were provided with this common convenience and with a table of answers and if the pages were less crowded and the margins not so narrow, the size of the volume, which contains fourteen chapters including three dealing with differential equations, would more nearly agree with its scope. Even then, however, the book would belong, where the author doubtless intended it should belong, to the class of text-books which in order to distinguish them from their more comprehensive and cumbrous rivals, are sometimes described as thin. It is hardly to be imagined that, among those extra-scientific features of a book that may properly be considered in determining its acceptability as a text-book for classes, mere thinness could count for much. Certainly a slight difference of length, breadth, thickness or weight could not be decisive. Perhaps the most competent teachers are apt to prefer the thin text-book as less likely to dishearten and overwhelm the beginnner by multiplicity and as leaving more room for personal view-point and individuality; but then the least competent, too, are, for obvious reasons, prone to the like preference. In this case the thin book will prove friendly to sciolism rather than to knowledge, as the student will hardly escape the impression that the science is as thin as the book.

The English is in general clear, precise and correct. The style is, however, uniformly dry, the reader being soberly conducted through the 'enchanted realm of open mystery' with scarcely a change of mood or variation of pulse. Books for boys, however logical and scientific, one could wish might be more vital and vitalizing, more human, more cheerful and sympathetic, addressed not so exclusively to the analytic and formularizing powers, but to the appreciation also, to the faculties of estrangement and curiosity, of wonder and admiration, looking not less towards knowledge but more towards culture. But even if we may not rightfully expect inspiration, we may, at all events, demand direction, orientation, judicious accentuation. The author does occasionally remark that some notion is fundamental, but in general the student is left to his own resources for discriminating the more from the less important matters. Cardinal theorems, at least, might have received the common emphasis of italics.

The first hundred pages are concerned with algebraic functions, which, by the way, again receive the old definition. Transcendental functions follow. Integration, in which considerable use is made of trigonometric substitutions, is throughout treated simultaneously with differentiation. By this arrangement, it became possible to introduce at an early stage a goodly variety of physical, geometric and engineering problems which serve to illustrate the practical utility of the calculus. In addition, by way of encouraging practice, numerous exercises, invariably called problems, have been inserted.

The author desires 'by a logical presentation of principles to inspire confidence in the methods of infinitesimal analysis.' It is really not at all necessary. College atmosphere is saturated with belief in the validity and power of this subtile analysis. The faith is acquired by a kind of 'cerebral suction.' The average student has too much of this 'confidence' even before he begins the study, and as a rule too much also at the end. A genuine intellectual conviction, though it may not follow doubt, certainly can not precede it; and the rigorist's first object would seem to be not to inspire, nor to preserve, but rather to mitigate the student's unearned confidence. To beget a wholesome skepticism is an indispensable preliminary, but this is as little undertaken in this book as in the majority of its competitors.

It should by no means be inferred that the book is devoid of modern elements. The notions of absolute and uniform convergence, for example, are introduced, and the conditions for term-by-term integration and differentiation of power-series are considered; but the work is not preëminently 'logical.' The infinitesimal

is regarded as 'a quantity which becomes indefinitely small.' According to the definition of limit, page 3, either of two *variables*, as $\frac{1}{2^n}$, $\frac{1}{3^n}$, n increasing, may be the limit of the other. On page 10 we are told that when a point, moving continuously on a given curve, passes a specified point of the latter, it tends at that instant to move on the tangent. There is, of course, no such tendency, and continuity is not defined till several pages later. On page 25 the conclusion, 'Hence the first derivative, etc.,' is, as stated, entirely unwarranted by the premises. 'Any finite constant' is much too sweeping. It appears to be assumed throughout that continuity implies derivability. The explanations of the differentials dy and dx, pp. 39, 40, are interesting and curious. It would be supererogatory to give here an exhaustive enumeration of the peculiarities encountered, the foregoing specimens, taken at random, being perhaps sufficient.

The final three chapters present plainly and pleasantly an introduction to the practical phase of differential equations. The existence theorem, naturally not proved, is however *tacitly* assumed, and such fundamental questions as whether all modes and orders of elimination lead to the same equation are neither met nor propounded.

Dr. Murray's book is a simple, fresh, luminous and suggestive presentation of the elementary subject-matter of the integral calculus. While it was written primarily for engineering classes and particularly adapted to conditions prevailing at Cornell University, still the needs of others have been regarded and the work is not ill-suited as a guide to any one beginning the study of this branch of mathematics. The first two chapters, in particular, furnish an unusually full account of fundamental concepts and operations. The two conceptions of integration, as the inverse of differentiation, and as a process of summation, are shown to be one. On pages 9 and 11 and elsewhere, the symbol $\int$ is spoken of as denoting now a sum and again the limit of a sum, with seeming indifference.

Chapter XII. deals briefly with the important subject of integral curves, and in the next chapter, which is final, we find a brief discussion of some common and important differential equations.

For the convenience of any who may not have the time—several months, at least—necessary for the mastery of all the matter offered, a list of lessons for a shorter course is suggested. Many other minor features help to enhance the acceptability of the book. The exercises are numerous and many of them are not found in other works. A table of answers is appended as also a short table of integrals. Binding and paper are substantial and printing and proof-reading well done. There is no great pretense of rigor but there is life. The book was not stillborn.

C. J. KEYSER.

COLUMBIA UNIVERSITY.

GENERAL.

THE Adjutant-General's office of the War Department has undertaken the issue of a 'French-English Military Technical Dictionary,' compiled by Cornélis De Witt Willcox, first lieutenant of artillery, U. S. Army. The first part, which has just been issued, contains 160 pages and reaches the word *espace*. The book is clearly printed with a judicious use of block and italic type. It will prove useful not only in the army and navy, but also to students of science in different directions. Many of the words translated will not be found in a good French dictionary as 'Littré et Beaujean,' yet they occur in scientific books. Not many Americans could give the equivalent of words such as *abouement*, *abougri*, *abraquer*, etc., and it is convenient to have at hand a dictionary in which they can be found. It is a matter for congratulation that there are in the army officers capable of such good scientific and literary work, and that it is encouraged by the authorities.

THE Experiment station of West Virginia University has recently issued Bulletin, No. 56, prepared by Dr. Hopkins, summing up the work done by him as entomologist of the Station during the past eight or nine years. This is a large bulletin of over 360 pages, and contains much valuable data collected by the entomological department during the time named, as it

gives the results of investigations, both in this country and Europe, relative to the spruce and pine interests, and the insects predaceous and beneficial to them.

MESSRS D. APPLETON & Co. announce the following new volumes in their library of 'Useful Stories:' 'The Living Machine,' by Professor H. W. Conn; 'The Alphabet,' by Edward Clodd and 'Organic Chemistry,' by Professor G. F. Chambers.

THE continuation of several important publications by The Macmillan Company, namely the second volume of the 'Scientific Papers,' by John Couch Adam; the third volume of the text-books of 'Embryology of Invertebrates,' by Drs. E. Korschelt, K. Heider, and the second part of the translation of Professor Van Zittel's text-book of paleontology.

BOOKS RECEIVED.

Liquefaction of Gases. WILLET L. HARDIN, New York and London. 1899. Pp. viii + 250. $1.50.

The Kinetic Theory of Gases. H. S. BURBURY, Cambridge. At the University Press. 1899. Pp. viii + 157.

The History of the European Fauna. R. F. SCHARFF, London, Walter Scott, Ltd. 1899. Pp. 364.

Untersuchungen über die Vermehrung der Laubmoose durch Brutorgane und Stecklinge. CARL CORRENS, Jena, Fisher. 1899. Pp. xxiv + 472. 15 Marks.

Annuario publicado pelo observatorio do Reio de Janeiro, 1899. Rio de Janeiro. L. MALAFAIA, JR. 1899. Pp. x + 318.

Das Tierreich. 5 Lieferung, *Sporozoa.* ALPHONSE LABBÉ. Berlin, Friedländer. 1899. Pp. xx + 180. Subscription price, 8.80 Mark. Single 12 Mark.

Catalogus Mammalium tam Owentum quam fossilium. E. L. TROUESSART. Fasciculus VI. Index, alphabeticus, Berolini, Fiiedländer. 1899. Pp. 1265–1469.

Electric Motive Power. ALBION T. SNELL. New York, D. Van Nostrand; London, The Electrician Printing and Publishing Co., Ltd. 2d Edition. Pp. vi + 403. $5.00.

Methods of Knowledge. WALTER SMITH, New York and London, The Macmillan Company. 1899. Pp. xxii+340.

Hand-book of Optics. WILLIAM NORWOOD SUTER, New York and London, The Macmillan Company. 1899. Pp. viii+209. $1.00.

Inorganic Chemical Preparations. FELIX LENGFELD. New York, The Macmillan Company. 1899. Pp. xviii+55.

Naturæ Novitates. Berlin, Fredländer. 1898. Pp. 780. 4 Mark.

The University Geological Survey of Kansas, Vol. V. Special Report on Gypsum and Gypsum Cement Plasters. G. P. GRIMSLEY and E. H. S. BAILEY, Topeka, J. S. Park. 1899. Pp. 83.

Mineral Resources of Kansas. ERASMUS HAWORTH, Lawrence, University of Kansas. 1899. Pp. 128.

Sextus Empiricus and Greek Scepticism. MARY MILLS PATRICK. Cambridge, Deighton, Bell & Co.; London, George Bell & Sons. 1899. Pp. viii + 163.

SCIENTIFIC JOURNALS AND ARTICLES.

American Chemical Journal, August. 'On Nitromalonic Aldehyde': By H. B. Hill and J. Torrey, Jr. 'Contributions to the Study of Aqueous Solutions of Double Salts': By H. C. Jones and N. Knight. The evidence is in favor of the view that the double chlorides exist as such in concentrated solutions and are only dissociated at great dilution. 'On the Rearrangement of the Thiocarbamic Esters:' By H. L. Wheeler and B. Barnes. 'Dimethyldianthracene, A Polymeric Modification of β-Methylanthracene': By W. R. Orndorff and H. A. Megraw. 'The Action of Chromic Acid on Hydrogen': By C. L. Reese. Experiments carried out under various conditions and temperatures showed that hydrogen is oxidized only very slightly, if at all, below a temperature of 100°. 'Action of Sulphuric Acid on Nitroheptane': By R. A. Worstall.

J. E. G.

THE *National Geographical Magazine* for September contains the following articles:

The Commercial Development of Japan, by O. P. Austin.

Bad Lands of South Dakota, by N. H. Darton.

The West Indian Hurricane of August 7–14, 1899, by E. B. Garriott.

The Return of Wellman, by J. Howard Gore.

The International Cloud Work of the Weather Bureau, by Frank H. Bigelow.

The number also contains several shorter articles, as 'The Rediscovery of Porto Rico,' 'Through Franz Josef Land,' and 'The Isthmian Canal Route,' besides a good deal of geographic miscellanea.

DISCUSSION AND CORRESPONDENCE.

NATURALISM AND AGNOSTICISM.

CIRCUMSTANCES, which need not be detailed here, having led me to pay somewhat careful attention to Professor Ward's most skillful 'Gifford Lectures,' I read Professor Brooks' review (SCIENCE, September 1st) of this work with keen interest. The notice cannot be termed unfair, unless, indeed, one take exception to the superfluous statement, "nothing is easier than for one who is not a naturalist to improve upon the work of Charles Darwin." Nothing in Ward's attitude, except, possibly, his tremendous castigation of Spencer, warrants such harshness. On the other hand, Brooks' entire outlook is so different, and the position he adopts so far removed from that of his author that there is a real danger lest readers of SCIENCE should tend to misprize a book wrought out, not only with remarkable analytic insight, but also in competent familiarity and sympathy with scientific methods. I cannot find that Brooks anywhere indicates what task precisely Ward attempts; on the contrary, he sometimes blinks the issue. And yet, this may be stated with directness, and without disrespect to the reviewer, which, I need hardly say, is far from my mind.

The advance of science in the eighteenth and nineteenth centuries has gradually crystallized into four theories, not of scientific phenomena, but of the universe as a whole. (1) The Mechanical Theory. This founds on abstract Dynamics, which deals with molar phenomena; on Molecular Mechanics, which is concerned ultimately with ideals of matter; while, latterly, Mechanical Physics has tended, in some hands, to give way before Energetics, which regards all change as either a transference or a transformation of energy. (2) The Theory of Mechanical Evolution, which seeks to trace *back* the phenomena of the universe, as they now are, to an original condition that can be expressed according to purely mathematico-physical formulæ—the theory of Spencer. (3) Biological Evolution as *implied* in the work of Lamarck, C. Darwin and their followers. (4) The Theory of Psychophysical Parallelism, involving Clifford's 'mind-stuff,' the 'double-aspect' theory, the 'conscious automaton' (Huxley) theory and, generally, the view that 'mind' is an epiphenomenon of 'matter.' The task essayed by Ward may be put in the form of the following question: Taking the fundamental conceptions employed by the various exponents of these theories, what can they be shown to involve when subjected to the analyses of Epistemology? In other words, to what conclusions do they lead inevitably, and are these conclusions sufficient to account for all that is actually involved in man's universe? Brooks' hint of dogmatism may be traced to an incomplete acceptance of the fact, fully accepted by Ward, that, for man, there is no universe but man's universe; and here all dogmatism is out of place.

So far as 'simple-minded men of science' are concerned, I think we may admit that Ward has exploded, beyond peradventure, the assorted dogmas peculiar to the first, second and fourth of these theories of the universe. I am by no means sure that he has achieved similar success with the third, possibly because it still remains so fluid, and I have a tolerably strong conviction that his constructive alternative, termed Spiritual Monism, will prove as unsatisfactory to others as to Brooks. At the same time, one must remember that he has stated this in the briefest and, therefore, most tentative fashion.

Brooks' review dwells almost exclusively on the third theory and, consequently, he hardly does justice to Ward's positive achievement; while, further, his difficulty in adapting himself to the epistemological standpoint seems to lead him to attribute to Ward positions which his author is far from holding. The sections of the review dealing with figurative language show this. The former lapse may be omitted as unimportant. The latter calls for some notice. The reason for Brooks' difficulty in envisaging Ward's standpoint comes out plainly in the following statement: "The naturalist agrees with Ward that our conception of the order of nature is not absolute, but contingent or relative, but he is not prepared to assert that it is a hypothesis; for a hypothesis is a mental product, and he does not know whether the contingency is mental or organic." Waiving the question whether there possibly *can* be an *order* of nature distinct from our conception

of it, this statement implies that there is *a* mental and *an* organic sphere, which may be treated as if each stood in isolation from the other. Whether such an idea be compatible with the Theory of Evolution appears very problematical. Be this as it may, the precise problem of Epistemology is just the question, *can* there be any sphere *for man*, in which anything may be regarded as if it were out of relation to mind, or to 'the mental,' using the more abstract language supplied us? Till this has been determined—and many advance valid reasons for concluding that it has been determined in the negative—discussion of 'teleology' and the like is so much beating the air.

But, fortunately, there happens to be far more community between Brooks and Ward than the printed page reveals. That Brooks should be moved to consider Ward's book at all, that he should attack some of the questions so significantly discussed in his brilliant 'Foundations of Zoology,' and that Ward should go entirely to the positive sciences for his materials are right hopeful signs of the times. No doubt Brooks' review bears witness to an appreciable remnant of that estrangement between science and philosophy which was at its height in the sixties and seventies. In the seventeenth and eighteenth centuries Descartes, Spinoza, Leibniz and Kant drew their materials from the sciences as then formulated; and the 'plain historical way' of Locke, and to some extent of Hume, commended itself to the sober methods of scientific inquiry. But at the beginning of the nineteenth century, thanks to the new 'social sense' that arose with Lessing and Herder and Goethe, philosophy forsook its commerce with the natural sciences and sought aid from the so-called human sciences, especially in those aspects which may be lumped under the name *Culturgeschichte*. This movement reached its zenith with Hegel and his followers. Meanwhile, the natural sciences, particularly in that development of them which Brooks ornaments, had themselves taken up and projected along new lines the very suggestions of the *Culturgeschichte* group, and had summed the results in the term Evolution. This term, as we now understand it, is no more than half a century old, a brief period in the life of any great operative conception, and we are far from clearly perceiving all it implies. "There is 'something more' at work," as Romanes said to me time and again. Ward's book is a product of this conviction of ignorance, so is Brooks' review. Further, the book must be taken as a powerful witness to the return of philosophy to the old, amicable relationship of the seventeenth and eighteenth centuries. The pressing affair of philosophy is to elicit the implications of theories which are not simply provisional groupings of phenomena scientifically observed, but profess to be *Weltansichten*. Just because they are at once scientific and philosophical, neither the scientist nor the philosopher can deal with them in his own corner. Brooks and Ward are at one in proving this. Indeed, the most interesting—some would say the most promising—factor in contemporary intellectual activity crops out in the fact that scientists are becoming more and more alive to philosophical problems, while philosophers are beginning to discover that, after all, their main concern is with the fundamental conceptions incident to that highly organized portion of human experience which goes by the name of science. Each side will better the prospect for a more thoroughly rational explanation of things known and to know by foregoing its own *idola*.

I should not have ventured to intrude at this 'great assize' but for the fact that Brooks attributes to Ward *idola* from which the Cambridge epistemologist has shaken free. On the other hand, and far more important, Brooks himself has already escaped many others which, in the not very distant past, generated that amazing hybrid—a mechanical biology.

R. M. WENLEY.
UNIVERSITY OF MICHIGAN.

THE ORIGIN OF MEASUREMENTS.

TO THE EDITOR OF SCIENCE: My small boy, aged 5 years, was discovered this summer to have originated a system of measurement which he used in conversation with other children. Certain distances were described as four men, and certain other distances were spoken of as a boy or half a boy. Certain others were spoken of as two men and a boy. Perhaps this may

be of interest in connection with the origin of measurements by the foot, the span, the hairsbreadth, etc.

H. H. CLAYTON.

BLUE HILL, MASS., September 5, 1899.

THE FAUNA OF PORTO RICO.

TO THE EDITOR OF SCIENCE: It is somewhat surprising to find in the current number of SCIENCE (Sept. 1, p. 286), a paper by Dr. Mark W. Harrington on the 'Fauna and Flora of Puerto Rico,' which shows the writer to have, in some respects, less knowledge of West Indian mammal and bird life than was possessed by the discoverer of these islands. Columbus, in his journals, comments on the absence of large animals in the islands which he visited and states that the only land mammal found was the Hutia, or Utia, on which he was feasted by the natives of the Bahamas, Hayti and San Domingo, and Cuba. In the last named island the animal is still common under this name,* three species being known, viz., *Capromys pilorides*, *C. melanurus*, and *C. prehensilis*. The remaining members of the genus are *Capromys brachyurus*, of Jamaica, now supposed to be on the verge of extinction, largely through the ravages of the Mongoose; *C. thoracatus*, a nearly allied form discovered by Townsend in Swan Island, and the remarkably distinct *C. ingrahami*, described by Allen from the Plana Keys, Bahamas, in 1891, when for first time Columbus' mention of the Utia in the Bahamas was given a scientific status. In Hayti and San Domingo there occurs a member of the same Histricomorphine family (Octotontidæ), *Plagiodonti aædium*, an exceedingly rare animal of which little is known, and this, with the six species of *Capromys* named, two species of *Solendon*—one each from Cuba and Hayti—and a small species of *Oryzomys* from Jamaica, constitutes the entire known indigenous terrestial mammalian fauna of the Greater Antilles; there being, therefore, no indigenous land mammal recorded from Porto Rico. For this reason it is with no small interest we find your correspondent saying of the 'wild fauna' of Porto Rico: "Generally speaking, the largest wild mammal is a ground squirrel, about the size of a gopher. A few others of larger size are reported from time to time, but they are only occasional and are probably animals escaped from cultivation. Probably the larger animals once existed, and their traces could doubtless be found by a linguist in the place names which abound all over the island and are quite often not Spanish * * *."

*In Hill's recently published 'Cuba and Porto Rico' (p. 55), this animal is miscalled 'Agouti.' Only one species is said to occur in Cuba, and the creature is stated to be found in the Windward Islands, but not in Jamaica, whereas the reverse is true. There is, however, in the Windward Islands a true Agouti (*Dasyprocta cristata*), the only member of the genus occurring in the West Indies.

The 'squirrel' mentioned is as yet unknown to students of the Greater Antillean fauna, who have also failed to discover, either in the records of man or nature, any evidence of the former existence of large mammals in these islands.

In respect to birds, it appears that both your correspondent and Columbus found 'Nightingales' in the West Indies; an error as pardonable 400 years ago as it is inexcusable to-day.

FRANK M. CHAPMAN.

AMERICAN MUSEUM OF NATURAL HISTORY,
September 7, 1899.

METHODS FOR A CARD INDEX.

IN the last number of SCIENCE Professor Porter, of the Harvard Medical School, outlines a plan for a card *Centralblatt* of physiology, which when carried into effect will greatly smooth the way for students of physiology and related sciences. I am not, however, sure that the plan proposed is the most practicable. A card index is without doubt the most convenient form of an index, chiefly because it can be continually and homogeneously increased. It is, however, bulky and somewhat inconvenient to use, and hence, I think, not suited for the publication of abstracts, especially when they extend beyond the limits of a single card. The most convenient and economical method of storing printed matter is in the form of books on a shelf. The card catalogue should be an index to these books.

There should be for each of the sciences *Centralblätter* or series of abstracts and probably one in each leading country so as to secure

completeness and different perspectives. Then there should be a central bureau as planned by the Royal Society, which would send out promptly a card catalogue giving all the titles and also references to reviews and abstracts (at least in certain standard journals), as they appear. The slips giving data regarding reviews and abstracts would, of course, refer to the article abstracted, and should, perhaps, be printed on narrow and thin slips which could be pasted on the original cards. If the abstracts in question are by competent men of science, it would be an advantage if an opinion were expressed in regard to the importance of the work reviewed, whether it is a compilation or an original research, etc. If this were done by some uniform system it could be carried over to the slip by a symbol, as a letter or a single word.

We are undertaking to carry out this plan for psychology in the Psychological Laboratory of Columbia University, but it has, of course, only local usefulness so long as the Index is not published. We have a card catalogue of psychological literature, and the card indicates whether the publication can be found in the University Library and if not the most accessible library in which it can be found. It is proposed to add references to abstracts and reviews, as far at least as they are contained in the *Zeitschrift für Psychologie* and the *Psychological Review*, and to indicate the character and value of the publication. To learn the contents it is only necessary to turn to a journal within arm's reach.

J. MCKEEN CATTELL.

COLUMBIA UNIVERSITY.

NOTES ON INORGANIC CHEMISTRY.

THE investigations of Professor K. A. Hofmann have shown the decided analogies which exist between hydroxylamin NH_2OH and water especially in possessing both a basic and an acidic nature. The basic nature lies in the tendency of the amido group to form an ammonium group, while the acidic nature rests in the hydroxyl group, in which the hydrogen atom is in derivatives replaceable by a metal. A new analogy between hydroxylamin and water is now shown by Rudolf Uhlenhuth in Liebig's *Annalen.* When hydroxylamin is added to a concentrated solution of nickel sulfate, a red crystalline precipitate is formed, which has the formula $NiSO_4$, $6NH_2OH$. This would be ordinarily considered hydroxylamin of crystallization. Nickel sulfate, however, crystallizes as many other vitriols with $7H_2O$. According to Werner's hypothesis one of these water molecules is united chemically with the SO_4, while the other six are coördinated with the nickel atom. Now the hydroxylamin could not be thus united with the SO_4, hence we find only six molecules present. This would seem to add another to the not long list of substances such as water, ammonia, etc., which can be coördinated with the metallic atoms.

Practical use is being made of the high temperature developed by the reduction of metallic oxids by aluminum, as described by H. Goldschmidt in the *Zeitschrift für Electrochemie.* Carbon-free metals are readily obtained, as chromium for chrome steel and manganese for manganese bronze. Vanadium oxid is reduced by aluminum only to the suboxid V_2O, but columbium oxid is reduced to the metal. As a by-product in these reactions an artificial corundum is obtained which surpasses the natural emery as an abrasive. When a mixture of iron oxid and aluminum reacts, the temperature is intense but is very circumscribed, so that it can be used for many purposes, such as welding steel, where a high temperature is desired locally.

A CONTRIBUTION to the chemistry of matches has appeared in the *Bollettino chimico-farmaceutico* by Giovanni Craveri of Buenos Ayres. He suggests the replacement of phosphorus in matches by perthiocyanic acid $H_2C_2N_2S_3$, and claims that such matches are not poisonous nor explosive, strike on any surface and burn brightly. Perthiocyanic acid can be readily made from the by-products of several processes, such as the purification of coal gas or the Lebane soda manufacture, and already its cost is less than that of phosphorus. If the new matches prove all that is claimed for them, Craveri will be recognized as a benefactor of the human race.

THE paper by Sir William Crookes on victorium, a new element associated with yttrium, recently read before the Royal Society has been

printed in full in the *Chemical News*. The discovery of the element, to which at first the name monium was given, resulted from photographic researches on phosphorescent spectra, it giving a very characteristic group of lines in the ultra-violet. The concentration of victorium is accomplished first by the fractional decomposition of the mixed nitrates of the yttrium metals by heat. The nitrates of the earths of the cerium group decompose more readily, and those of the yttrium group less readily than that of victoria, so that after a large number of fractionations the victoria collects in the middle portions. These middle fractions are then submitted to fractional precipitation with oxalic acid, many times repeated, and finally the portions richest in victoria are converted into sulfates and fractionally precipitated with potassium sulfate. In the purest condition thus far obtained, victoria is a pale brown powder, less basic than yttria and more basic than most of the oxids of the terbia group. Assuming the oxide to be Vc_2O_3, the atomic weight of victorium is about 117. The most marked characteristic of victoria is its spectrum.

J. L. H.

ZOOLOGICAL NOTES.

IN the annals of the South African Museum, Mr. L. Péringuey describes a method, discovered by Rev. J. A. O'Neil, for capturing both sexes of the members of the hymenopterous genus *Mutilla*. By seizing the female in such a way as to induce her to produce her well-known stridulation, the males immediately appear and are easily secured, at times even settling on the hand of the captor. As the sexes are certainly known in but 16 out of the 169 South African species, the practice of this 'sembling' method, as it is styled, is to be recommended.

THE report of the Australian Museum for 1897 records the mounting of a specimen of the Galapagos tortoise *Testudo nigrita* brought to Sydney, New South Wales, by the American whaler *Winslow*, in 1853. At that time it weighed 53 pounds, while at the time of its death, in 1896, its weight had increased to 368 pounds, a more rapid rate of growth than such animals are usually credited with.

ACCORDING to Mr. Etheridge of the Colombo, Ceylon, Museum, by far the largest cobra ever recorded is one measuring 7 feet 9 inches taken at Jaffna, but as the measurement was made on a skin, it is possible that the maximum length attained by this deadly snake is not far from 7 feet 6 inches.

MR. ETHERIDGE discusses the use of formol at some length, stating that its great fault is its bleaching property, and that pure glycerine can alone be trusted to keep color, because it excludes those great destroyers of animal colors, air and water. Formol in combination with various salts will preserve color for a greater or less length of time, but not permanently. Thus a three per cent. solution of formol, saturated with common salt, preserved the color of *Oreastes turitus* for about eighteen months, and then the specimen faded completely in a few days. Epsom salt in combination has the curious property of keeping the fugitive blues, greens and violets of the wrasses for at least a year, although destructive to the colors of other fishes.

IT will doubtless surprise many to be told that the mastodon is far more common in American museums than is the African elephant. The skeleton of Jumbo in the Am. Mus. Nat. Hist., New York City, is almost the only specimen of this animal in the country, while there are at least ten mounted skeletons of mastodon and teeth and bones without number. It is not too much to say that not a week elapses without some published account of the discovery of mastodon remains and while most of the specimens are poorly preserved, or consist only of individual teeth, yet in the aggregate their number is very considerable. Orange and Ulster counties, N. Y., appear to have been favorite burying places for the mastodon, and from the character of the ground it is evident that many specimens will yet come to light from these localities.

F. A. L.

SCIENTIFIC NOTES AND NEWS.

THE Astronomical and Astrophysical Society of America, which, as we have already stated, was recently established at the third Conference of Astronomers and Astrophysicists held at the

Yerkes Observatory, has elected officers as follows:

President, Simon Newcomb; Vice-Presidents, C. A. Young, George E. Hale; Treasurer, C. L. Doolittle; Councillors for two years, E. C. Pickering, J. E. Keeler; Councillors for one year, E. W. Morley, Ormond Stone; Secretary for three years, George C. Comstock.

We hope to publish in an early issue a full report of the meeting at the Yerkes Observatory together with abstracts of the papers presented.

IT is expected that the New York Zoological Park will be formally opened to the public during the second week in October. Only a few of the buildings will be completed, but there is already a fairly representative collection of animals in the Park.

THE United States Fish Commission steamer *Albatross* sailed from San Francisco on August 23d, with a scientific party under Professor Agassiz bound for the South Pacific. The objects, itinerary, and personnel of the expedition were noticed in the issue of SCIENCE for June 9. The voyage will occupy eight or nine months, and is expected to yield much valuable information pertaining to the fauna of the little-known regions that will be visited.

DURING the present season the U. S. Fish Commission has had a number of field parties, in various States, engaged in ichthyological and other investigations. A camping party under the direction of Dr. Charles H. Gilbert has systematically examined the coastal streams of Oregon, with reference to their fish fauna; the eastern tributaries of the Sacramento have been visited by Mr. Cloudsley Rutter; a comprehensive study of the biological and physical features of the Wabash basin has been begun under the direction of Professor B. W. Evermann, who is assisted by Dr. J. T. Scovell, Dr. C. H. Eigenmann and others; a party in charge Mr. W. P. Hay has explored the Monongahela basin in West Virginia; Dr. P. H. Kirsch has been collecting and studying the fishes of the San Pedro River, Arizona; in connection with the biological survey of Lake Erie, Professor Jacob Reighard and assistants have cruised along the northern and southern shores of the lake in a special steamer; Dr. H. M. Smith has visited Seneca Lake, N. Y., for the purpose of determining the character of its fish fauna; a study of the variations of the mackerel of the east coast has been conducted by Mr. M. C. Marsh, and in the interesting Sebago and Cobbosseecontee lake regions of Maine, Dr. W. C. Kendall has made some special investigations regarding salmon and other fishes.

WE regret to learn that Professor E. W. Hilgard, of the University of California, and Director of the California Agricultural Experiment Station, has been seriously ill during the summer. It is feared that he will not be able to resume his duties at the beginning of the academic year.

MR. W. T. SWINGLE, agricultural explorer for the Department of Agriculture, has returned from an extended trip to the Mediterranean countries, undertaken for the Department, with the view of finding new agricultural industries capable of being introduced into the United States. He made a special study of viticulture and of the date and fig industries, and for some months will be at Washington, engaged in preparing for publication the results of his trip.

PROFESSOR G. S. FULLERTON, who holds the chair of philosophy at the University of Pennsylvania, has returned to Philadelphia after a year's absence in Europe.

PROFESSOR C. H. HITCHCOCK, of Dartmouth College, will resume his work this month after a year's leave of absence spent in Australia and Hawaii where he has been carrying on geological work.

DR. THURSTON, of Cornell University, has been requested to serve on a number of the Congresses, to be held in connection with the Paris Exposition, including those for mining, metallurgy, testing materials of construction and applied mechanics. He has been appointed a member of the *Comité de patronage*, and has been invited to prepare the reports on 'Mechanical Laboratories,' as *rapporteur* or editor and the introductory paper. He particularly desires full accounts of all laboratories of that character in the United States. Men of science, interested in the subject, either through their

connection with the physical sciences, pure or applied, or as engineers interested in research in these departments, who wish to join this Congress may apply either to Dr. Thurston or to the Secretary of the American Society of Mechanical Engineers for circulars giving the form of organization and a statement of the questions to be discussed, as well as for cards of 'adherence' to the several divisions of this Congress.

DR. R. BURCKHARDT, professor of paleontology at Bâsle, and Dr. V. Uhlig, professor of geology in the German Technical Institute of Prague, have been elected members of the academy of sciences at Halle.

THE Physiological Institute of the University of Berlin, has been presented, by his widow, with a marble bust of Emil Dubois-Reymond.

AMBROSE P. S. STUART died at his residence in Lincoln, Nebraska, September 13, 1899. He was born November 22, 1820, in Sterling, Worcester county, Mass. He graduated from Brown University in 1847, with the degree of A.B., and spent three years subsequently at Heidelberg. He taught school for a number of years, and in 1865 became instructor in chemistry in the Lawrence Scientific School of Harvard University. Later he was professor of chemistry in the Pennsylvania State College, and still later in the University of Illinois. He removed to Lincoln, Nebraska, in 1875, where he engaged in business, amassing a considerable fortune. Throughout his life he maintained his interest in scientific matters, and despite his advancing years was a familiar figure in the meetings of scientific societies.

THERE will be, on October 17th, civil service examinations for the position of nautical expert in the Hydrographic Office of the Navy Depart with a salary of $1000, and for the position of ornithological clerk in the division of Biological Survey, Department of Agriculture, with a salary of $660. Candidates for the latter position should be between 20 and 25 years of age.

THE late Richard B. Westbrook of Philadelphia has made a bequest of $10,000, taking effect on the death of his widow, to the Wagner Institute of Science. The sum is to be used as an endowment of a special lectureship to "secure the full and fearless discussion by the most learned and distinguished men and women in our own and other countries of mooted or disputed questions in science, and especially the theories of evolution."

MR. ANDREW CARNEGIE has given $50,000 to the City of Oakland, Cal., the city having undertaken to guarantee at least $4,000 annually for its support.

THE schooner *Julia E. Whalen*, Captain Noyes, has arrived from a cruise to the Galápagos Islands and to Cocos and Clipperton Island west of Ecuador. The vessel had not touched any inland port since she sailed from San Francisco, October 30th, last. She carried members of a scientific expedition under direction of Robert E. Snodgrass, assistant in entomology and Edmund Heller, student in zoology, sent by Stanford University, under the patronage of Timothy Hopkins, of San Francisco. A large collection of specimens, including birds, mammals, invertebrates, and fish, was obtained. Aboard the vessel were eighteen live land tortoises taken from Duncan and Albemarle Islands, some of them weighing four hundred pounds; also 220 fur sealskins and 2,300 skins of hair seals.

A CABLEGRAM states that the British Association for the Advancement of Science at its present meeting has granted £1,000 toward the expenses of the British antarctic expedition.

THE steamer *Antarctic*, which left Helsingborg, Sweden, on May 25th last with an expedition under Professor A. G. Nathorst, was spoken off The Skaw, the northern extremity of Jutland, Denmark, on the 11th ult., on her return from her search along the northeast coast of Greenland for Andrée. No traces of the missing æronaut had been found.

INFORMATION has been received from the captain of the icebreaker *Ermack*, arrived in the Tyne, to the effect that he met the Prince of Monaco's yacht *Princess Alice* on August 21st, in Advent Bay, Spitzbergen. The yacht had been aground six days in the Red Bay, and after discharging 200 tons of coal and stores was floated. There is no leakage, but the vessel has received some small damage.

THE death has taken place at Leith of Mr.

John Ramsay, one of the survivors of the expedition sent out to search for Sir John Franklin. He joined the Navy in 1849, and formed one of the crew of the *Resolute*, which sailed from Woolwich in 1852.

On the 20th of August last an International Conference met at Berne to study the glaciers of the Rhone and Aar, to which extended excursions were made. Among those present were Professors Penck of Vienna, Reid of the Johns Hopkins University, von Drygalski of Berlin, Fürsterwalder of Munich, Baron von Toll of St. Petersburg, and other students of glaciers.

The Intercolonial Medical Congress of Australasia will hold its fifth session in Brisbane from September 18th to 23d.

The tenth congress of Italian Alienists will be held in Naples from the 10th to the 14th of October, under the presidency of Professor Tamburini. We learn from the *British Medical Journal* that the following are the subjects proposed for discussion: Practical methods of individual psychological investigation in asylums and clinics; the light which has been, and may be, thrown by anatomical data on normal and pathological psychology; psychiatry and the study of the individual and his activity in social relations; intoxications and infections in the pathogenesis of mental diseases and neuropathies.

The German Government has sent Professor Kossel, of the Board of Health, to Lisbon and Oporto to study the plague and the methods adopted to combat it. He is accompanied by Professor Frosch, of the Berlin Institute for the Study of Infectious Diseases, who is being despatched on the same mission by the Prussian Government.

UNIVERSITY AND EDUCATIONAL NEWS.

The Rose Polytechnic Institute will shortly receive $50,000 from a bequest of the late Joseph Collett, the payment of which has been delayed through litigation.

It is reported that the German technical schools will be authorized to confer the degree *Doctor rerum technicarum*, and that the degree will be conferred for the first time on the occasion of the celebration of the centenary of the Charlottenburg Technical School.

The Regents of the University of Texas have provided a psychological laboratory which has been placed under the charge of Professor Caswell Ellis, of the department of pedagogy.

At the University of West Virginia the following appointments have been made: Edward D. Copeland, A.B. (Stanford), Ph.D. (Halle), lately assistant professor of botany at Indiana University, to be assistant professor of botany; J. B. Johnston, Ph.D. (Michigan), to be assistant professor of zoology; Otto Folin, B.S. (Minnesota), Ph. D. (Chicago), to be assistant professor of chemistry, and J. D. Thompson, M.A. (Cambridge) of Trinity College, Cambridge, and University College, Sheffield, to be assistant professor of mathematics.

At the Ohio State University, W. E. Henderson has been appointed assistant professor of analytical chemistry and C. B. Morrey, assistant professor of anatomy and physiology.

A. Kirschmann, Ph.D., lecturer in philosophy at the University of Toronto since 1894, has been appointed professor of philosophy and director of the psychological laboratory.

Professor Richard Pfeiffer has been called from the Berlin Institute for infectious diseases to the University of Königsberg as successor to Professor von Esmarch.

It is reported that Dr. Arons, Privat-Docent for physics in the University of Berlin, has been called to a chair of physics at Würzburg. It will be remembered that Dr. Arons has been prosecuted by the Government for being a socialist, but that the philosophical faculty of the University of Berlin refused to take any action disciplining him.

Rollo Kent Beattie, B.Sc., 1896 and A.M., 1898, of the University of Nebraska, has been elected to the instructorship in botany in the Agricultural College at Pullman, Washington, and John Lewis Sheldon, B.Sc. (Nebraska), recently appointed assistant in botany in the same university, has accepted the instructorship in biology in the Nebraska State Normal School.

PEIRCE ON THE PERCEPTION OF HORIZONTAL AND OF VERTICAL LINES.